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RESEARCH

RESEARCH

THE PATHFINDER OF SCIENCE AND INDUSTRY

BY T. A. BOYD

*Research Division
General Motors Corporation*



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TO
FRANK ORVILLE CLEMENTS

Discovery and invention do not spring full grown from the brains of men. The labor of a host of men, great laboratories, long, patient, scientific experiment build up the structure of knowledge, not stone by stone, but particle by particle. This adding of fact to fact some day brings forth a revolutionary discovery, an illuminating hypothesis, a great generalization, or a practical invention.

—HERBERT HOOVER

FOREWORD

ONLY a few miles and a few years apart were born two of the world's great explorers, Christopher Columbus and Leonardo da Vinci. Columbus typifies the geographic explorer, Leonardo the scientific explorer.

By sailing westward into the "Sea of Darkness" and finding there a new continent, Columbus inaugurated the golden age of geographic exploration. But, because the world has now been pretty thoroughly explored, the long age of geographic exploration is fast drawing to a close. Already, for virgin territory, the orthodox explorer must push off among the blizzards of Polar seas. In the words of Admiral Peary, "The last great earth story has been told."

Not so with exploration in the realm of science, wherein Leonardo was a great pioneer. There the unexplored is still far greater than the explored, and it will likely continue to be so for a long time to come.

Leonardo da Vinci is not generally thought of as having been a man of science, to be sure. He is remembered for his marvelous work as creator of such artistic masterpieces as "The Last Supper" and the "Mona Lisa." But, besides his works of art, Leonardo left a log or record of his scientific explorations which totaled five thousand pages. These notes of his, excellently illustrated of course, but written all in reversed or mirror writing, are scattered now among European libraries. They disclose some things that are

generally considered as having been more modern discoveries.

In making experimental explorations of things in nature, however, Leonardo was too far ahead of his day. For a long, long time there was no one to carry on after him. It was not until comparatively recent times, in fact, that the kind of exploration in which Leonardo was a pioneer began to be pursued at all generally. And only within the present generation did such exploration reach its golden age—if even the present may be taken as being that. Whether or not this is its golden age, we are now definitely in a period of widespread exploration and experimentation, not only in science, but also in education, in agriculture, in manufacturing, in business, and in nearly every other field of human endeavor.

And so the pioneering spirit of the explorer has not deserted us at all. It has only shifted its sphere of application. What has happened is that the restless ship of the geographic explorer has been replaced by the quiet laboratory of the up-to-date explorer. The galvanometer has taken the place of the compass. The telescope, the microscope, and the spectroscope have displaced the spy-glass. "The great adventures to-day," said Frederick Houk Law, "are the adventures of science."

The search of present-day explorers is for new knowledge about matter, about energy, about nature, about things in general; for ways of improving methods, materials, and mechanisms, and of making them more useful to people; for means of preventing and curing disease, of lengthening life, and of making life fuller, richer, and better while it does last. The old exploration, said Benton MacKaye, was

concerned with "*that which is*," while the new projects forward to "*that which can be*." It is about pioneering of this new kind—which is called research for short—that the following pages tell in a systematic manner.

T. A. B.

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RESEARCH

PART I

ORIENTATION

THE WIFE of a well known research worker tells that she has never yet been able to get her folks to understand just what it is that her husband does. Maybe one of the reasons is that she does not understand research very well herself. However that may be, her unsatisfactory experience in trying to explain research to others is a common one even among research workers themselves. People know, of course, what teachers, lawyers, doctors, farmers, bakers, butchers, and even racketeers, do. But they cannot well imagine how a research worker puts in his time.

Even among those actually engaged in research there are differences of opinion about what research is and what the limits of its definition should be. And so it seems appropriate that the text of Part I of this work should be concerned with what in the present instance is to be taken as the meaning and the scope of the modern form of exploration called research.

RESEARCH

CHAPTER I

DEFINITION¹

IN spite of the difficulties that often beset the doing of it, research is just the simplest, most elementary of things. It consists merely in searching for new knowledge or in trying to improve something. There are therefore many parallels to it, and even some actual instances of it, in everyday life. Who is there that does not search for new information on subjects that he is interested in? The village gossip—and even the city gossip who syndicates his stories in the newspapers—certainly seeks for new knowledge about the doings of people in the neighborhood. The newspaper reporter is constantly on the lookout for new information. He calls it *news*, and if he can get the story to his paper before any other investigator finds it out, then in his parlance it is a *scoop*. The detective traces down clue after clue in his patient search for new information. He calls it *evidence*. That great Italian “microbe-hunter,” Giovanni Grassi, who helped to find that it

¹ It should be said perhaps that the correct pronunciation of the word research is *re-search'*—accent on the last syllable. Research is quite commonly mispronounced by accenting the first syllable, *re'-search*.

is the anopheles mosquito that spreads malaria, compared himself to a policeman trying to find the criminal in a village murder.

The cook who experiments in the effort to improve old recipes or to develop entirely new ones is doing real research. To find out how to make the kind of candy called fondant, for instance, don't look in a cook-book at all. Look in the *Journal of Physical Chemistry*, Volume 23, page 589. Related there are the results of research on fondant-making by Mary Stevens Carrick, done under the direction of an outstanding colloid chemist, Professor Wilder D. Bancroft of Cornell University. For directions on how to make the best coffee read the reports of exhaustive researches on coffee-making by Professor Samuel C. Prescott, head of the Department of Biology and Public Health, Massachusetts Institute of Technology.

The man who stands on the sidewalk all day long counting the passers-by in order that a chain store may find the most likely location for a new retail outlet is likewise doing research, for he is gathering information which would not be available otherwise. That is research just as truly as is the effort of the chemist in the laboratory who is trying to find the conditions under which some chemical process will give its best yield. Willis R. Whitney, talking to the girls at Vassar about the new woman, said, "Let her experiment, and, whether the researches be on dresses or eugenics, in politics or industry, the direction will be right." Re-

search is elemental and universal. It is applicable to any field of human endeavor.

This view of the meaning of research would, to be sure, be considered as too rudimentary by those who like to restrict the use of the word to investigations in the field which is commonly called science. But so to restrict the meaning of research appears to be no more justifiable than to say that only those who study physics, or chemistry, or mathematics, really study, and that a man who studies bricks, or bees, or butterflies, does not study. Science, according to R. A. Millikan, is after all merely the growth of man's understanding of his world, and hence of his ability to live wisely in it. Surely there is nothing abstruse or incomprehensible about that. It is perhaps because people commonly think of research as applying only within the field of conventional science that they do not understand it, for to most people "scientific" matters mean those that are obscure. Charles F. Kettering has said that "a scientific thing is just anything you don't understand. As soon as you do understand it, it isn't scientific any more." One of its readers wrote to *Collier's* asking whether the popular excitement attending the visit of Albert Einstein to the United States during the winter of 1930 was due to the passion for mystery stories current at that time.

The Royal Society of London, founded in 1662, was one of the first organizations devoted specifically to the form of exploration which to-day is called research.

Here are some of the subjects proposed for investigation at one of the earliest meetings of the Society:

Experiments with wires of severall matters of ye same size, silver, copper, iron, &c., to see what weight will breake them. Experiment concerning the force that presseth the aire into lesse dimensions; and it was found that twelve ounces did contract $1/24$ part of Aire. Experiment to show how much aire a man's lungs may hold, by sucking up water into a separating glasse after the lungs have been well emptied of Aire. Experiment of Animal engrafting, and in particular of making a Cock spur grow on a Cock's head. Whether there bee any such thing as sexes in trees and other plants.

Each one of the matters suggested for investigation in this quotation from the early records of the Royal Society is something that the members did not then know about, and that some member thought it would be good to find out about. The way they proposed to find out was naturally to "experiment." The word *experiment*, which occurs four times in the above quotation, is so fundamental in the field of research as to form both the basis and the definition of it.

"All human progress in the arts has necessarily been the result of experiment," says Frank B. Jewett. In most of our activities we have to draw conclusions from complex experiences. That is one of the chief reasons why it is that so many of the things people believe to be so are not so. In directed experiment, on the contrary, the fundamental rule is that, so far as is hu-

manly possible, conditions shall be arranged so that there is involved not several unknown factors; and not even two, but only one. This is done in order that when the answer is obtained it will be specific and precise. It is this pointedness or singleness of inquiry that forms the basis of the scientific method of exploration, and so of research in general.

Research then consists essentially of experimentation—of experimentation under conditions that are carefully controlled. It may be the making of accurate and minute measurements of things or events in nature. “Nearly all the grandest discoveries of science,” said Lord Kelvin, “have been but the rewards of accurate measurements and patient, long-continued labor in the minute sifting of numerical results.” Gathering facts, systematizing them, and then reaching such conclusions as the facts justify, that is one of the ways to do research.

Research may consist in making careful computations, whether they be concerned with the orbits of the stars, with the weight of the electron, or with how many subscribers a telephone company is likely to have to take care of in a given community ten years hence. For the latter purpose, there are on the staff of the American Telephone and Telegraph Company to-day some of the ablest mathematicians in the world. With mathematics as his tool, Albert Einstein has made four great contributions to the progress of science: first, the Brownian movement equation; second, the photo-

electric equation; third, the equation of the interconvertibility of mass and energy; and, fourth, the famous theory of relativity.

Research may consist of a search for new ideas or knowledge, or of the testing of the validity of ideas already advanced. Galileo went up on the leaning tower of Pisa to make a practical test of the validity of the then common belief that a heavy body falls faster than a light one. By showing there that weight has no effect upon the velocity of falling bodies, he disproved an idea held for centuries. No doubt the confidence which Galileo had as he went up the tower was based upon prior experiments conducted in the privacy of his laboratory.

Research may consist of the effort to improve something, whether it be a machine or a medicine. James Watt, by his classical experiments, transformed the crude and imperfect steam engine of his time into an effective means of developing power. As a result of the continual efforts of a host of experimenters, the motor car has gradually evolved from a mere horseless carriage with buckboard body and spindly wheels, whose most useful accessory was a tow rope, into the satisfactory, self-reliant, and beautiful car of to-day. Research has worked many improvements in the field of medicine, such as the discovery that even leprosy and syphilis can be cured; that diabetes can be controlled with insulin, and pernicious anemia with extract of liver; that germ diseases can be largely controlled by

sanitation, and that some of them can be immunized against and cured as well.

There are some who make a distinction between "research" and "development." Use of the word "research" is restricted in their meaning to investigations on what is thought of as the plane of science, or to explorations either on the frontier of current knowledge or beyond it. By "development" they mean that form of systematic experimentation which is aimed directly at improvement in the arts, and which in many instances adapts and uses the product of the more fundamental "research." What may appear to be a somewhat similar distinction is drawn in the next chapter in defining the difference between pure research and applied. But both forms of investigation are here called research, for both are thought of as really being research. After all, they differ more in intent than in character.

Having thus attempted to define in general terms what research is, it may be worth while to suggest something also about what research is not. Being a search for *new* knowledge, research is not, for instance, the gathering of conventional items of information about an already established product. The testing of materials to determine whether they meet certain specifications is thus not research. Research does not apply to a routine form of inspection such as that is, even though the actual carrying out of the tests themselves might be thought of as experimentation. As a parallel,

it may be said that the student of history is not doing research in his field so long as he confines his investigations to already trodden paths. Reading the prepared histories of Spanish discovery and conquest in America, for instance, is not research. But the man who goes to Seville, Spain, there to search through the thousands of rolls of original records of Spanish doings in the New World, most of which have as yet never been read by a historian, is doing research in American History.

It is often not possible to tell merely by visiting a research laboratory whether any pioneering research is being done there. All one can usually see as he goes through a research laboratory are the tools and the machinery of research. In some respects the physical equipment of a laboratory doing research of a pioneering nature is very similar to that of a laboratory which does nothing but the mere testing of existing products. Both the testing laboratory and the research laboratory have to *measure* things, you see. But there is a very real difference between the research laboratory and the testing laboratory. That difference is this: The testing laboratory concerns itself merely about *that which is already in existence*, while the research laboratory is concerned about *that which could exist*, if only there were enough imagination and knowledge to bring it into being.

It happens that the matter which follows deals most directly with research as applied to science or to industry. But that is not because research is not rudi-

mentary enough to be a universal thing. Nor is it because research is not already being pursued in a wide range of fields, such as in education, in religion, in politics, in psychology, in banking and finance, in manufacturing, in purchasing, in marketing and sales, and in advertising. Rather, it is because science and industry are the fields in which research has been and is most definitely and consistently pursued, and which therefore offer the greatest wealth of pertinent material for a treatise of this kind. But the same general technique and principles apply whether the research be concerned with finding the structure of a molecule, with how to make a new dye, or with means for improving the selling of goods at retail.

Every one, no matter what he does, needs to understand research. He really ought to go still further and actually run a research laboratory of his own. Not that he should keep up an institution manned by trained research workers at all, but rather that he himself should be experimentally minded. A person's beliefs and his actions should not be allowed to rest lightly upon mere opinion; they ought to be grounded as solidly as possible upon a base of verified fact. That is a thing which can be done by nothing short of some first-person research.

If each one of us should do a little research of his own when occasion demands, we would not all be so dead sure of a number of things that are not so. The things that people differ on and argue about are in

large measure those which are mere opinions. This is why the discerning Abe Martin once remarked that it is funny how a man with facts can break up an argument.

CHAPTER II

PURE RESEARCH AND APPLIED

THERE are two general kinds of research: pure and applied. Although, as Arthur D. Little said about chemistry, there is perhaps a third also: mis-applied. Both pure research and applied come within the scope of the definition given on the preceding pages. For the question whether research is pure or applied is, in the words of C. E. K. Mees, "merely one of intention."

The sole intention of the investigator in pure science is to advance human knowledge. Exploration in the field of pure science is thus an attempt to find out about something in nature for the simple sake of knowing. The intention of the applied science explorer is either to convert already discovered truth into practical usefulness, or else it is both to discover new facts and to apply them in a practical way.

Sir Humphry Davy was doing research in pure science when, at Genoa, with the help of the youthful Michael Faraday, he investigated the electricity of the torpedo fish; and also when, with the aid of the great burning glass in the Accad mia del Cimento in Florence, he burned a diamond and found from its products of combustion that the diamond is almost

pure carbon. But when he was investigating astringent vegetables for the benefit of the British tanning industry, and also when making the careful study of mine explosions which led him to the invention of the miner's safety-lamp, Sir Humphry was doing applied research.

The methods used by the investigator in applied science are in no essential wise different from those employed by the pure science explorer. It is the use that is expected to be made of the results that constitutes the essential difference between the two. The same kind of training and skill are requisite to both types of research workers.

The investigator in applied science has, however, one distinct limitation which the pure science explorer does not have. This is the necessity that he is under of producing a useful result or a practical solution of some definite problem, and of doing so just as early as possible. He cannot, therefore, do much of what is called "random" research. That is to say, he can not browse around at will in his assigned field, selecting for study only those problems that appeal to him most. He has been charged with opening up a definite passage through a designated area; and so he must tackle and overcome every obstacle in the pathway, whether it appeals to him as an interesting subject or not.

In drawing the distinction between research in pure and in applied science, it should not fail to be said that, in spite of intentions, the difference between the two

forms, so far as ultimate results are concerned, may not be very great. It is natural to suppose that the kind of information secured in any investigation should not always depend solely upon the intention of the investigator. And indeed it does not. An investigation that has nothing but a purely scientific intent may, and often does, yield information that is of great practical importance; and, in the same way, it frequently happens that results obtained by the practical investigator turn out to be of large scientific value.

It was as an endeavor in pure science that Mendeléeff made his famous periodic classification of the chemical elements. But his arrangement has since proved to be of the greatest practical importance to the chemical industries and to all workers in the science of chemistry. Some men who follow chemistry always have about them a periodic chart of the elements. Thomas Midgley, Jr., for instance, carries one in his pocket wherever he goes. Simply as a pastime and out of curiosity, Antony Leeuwenhoek experimented with making microscopes and with using them to look at microbes. From his random research sprang later on the greatest of practical benefits to mankind, notably in sanitation and in the control of germ diseases. J. J. Thomson doing research in pure science discovered the electron. Little he probably thought that before long the electron would be put to work in the vacuum tube. Nor did Monsieur and Madame Curie have the cure of cancer in mind while they were making their

patient search for radium. All of practical mechanics is based upon the work of Galileo and Sir Isaac Newton, carried out without any practical objective.

On the other hand, Pasteur, with his helpers Roux and Chamberlain, was stumbling along in his intensive search for a practical means of curing or controlling chicken cholera when he discovered the principle of immunization, a thing that is of great importance from a scientific as well as from a practical viewpoint. In the same way, Pasteur's investigations of yeast were undertaken in an effort to cure fermentation troubles for a man who was producing alcohol from beet sugar; but they led him on to the making of fundamental contributions to the science of fermentation. The practical research of Charles P. Steinmetz on the laws of alternating electric currents added much to scientific knowledge in that field. The great advances in mathematical science were made in the efforts to solve puzzling but practical mathematical problems, notably in the field of astronomy. So also the important Second Law of Thermodynamics came out of the practical attempt to make steam engines more efficient. It was while trying to get a better understanding of how the steam engine of his day worked that Carnot made the first imperfect formulation of this law, which appears to be one of the most important of all physical laws.

So many and so wide are the gaps in nearly every field of knowledge that rarely indeed can a job of applied research be completed without doing something

which, if done for its own sake, would be classed as pure science research. Every one who has had occasion to look for some needed item of information in such sources as *International Critical Tables* will know how true this is. Very often the particular constant that he needs and must have before he can go further, is not there. It has never been determined. And so he has to determine it himself, just as the pure science investigator would have done had he happened to need it.

The results of research are frequently so unexpected in character that from the nature of what an investigation yields it is not always possible to tell whether the intention of the investigator himself was merely to advance human knowledge (pure science research), or to discover something of practical application (applied science research). Irving Langmuir, trying to find out more about the so-called "Edison effect," which is a blue glow that sometimes appears on the filaments of incandescent lamps, was studying the emission of electrons from hot tungsten filaments. While doing this work he observed that the presence of a little thorium in the tungsten wires increased the number of electrons emitted by as much as ten thousand times. That discovery, which was at once a scientific and a practical one, was soon put to use in the low-voltage, high-power vacuum tube for long-range radio transmission. "There is no knowledge that is not power," said Emerson. And not as a usual thing does the one who generates power know all the uses people will find for it.

But here it is worth while remembering that power is of no use to anybody unless or until it is put to work at something.

It sometimes happens that the investigator in the field of pure science looks with scorn upon the work of the applied science investigator. This is perhaps partly because of his fear that whenever researches are conducted in a frank effort to produce something that will be useful to people the word is spelled with dollar signs: RE\$EARCHE\$. Sometimes on the other hand, it is the worker in applied science who disdains the research of the pure science investigator. "An impractical dreamer," the worker in applied research may call the pure science investigator; "a trouble-shooter," the pure science investigator may call the industrial research worker. "An inventor," it was said in *Fortune*, "is a person whom the scientist regards as a mechanic." But the truth of the matter is that society is lucky to have both kinds of investigators. Their efforts really supplement and assist each other in a wonderful way. "Let not, then, the devotee of pure science despise practical science," said Professor Henry A. Rowland in 1884, "nor the inventor look upon the scientific discoverer as a mere visionary person. They are both necessary to the world's progress and they are necessary to each other."

But, believing as they do that pure science research occupies a higher plane than research in applied science, there are some research workers who consider

it beneath their dignity to make any practical application of the results of their explorations. Fortunately, however, there are many more who do not. It was no less a scientific explorer than Lord Kelvin who said that "The life and soul of science is its practical application." Kelvin devoted much of his own scientific talent to such practical objectives as solving the problems of laying a transatlantic cable, and improving the sounding line and the mariners' compass. "I don't know who this Thomson may be," a grateful blue-jacket once said of Lord Kelvin, "but every sailor ought to pray for him every night." Even Archimedes did not think it beneath him to use his scientific knowledge in the construction of those military engines and devices of his which for three long years kept the Romans out of Syracuse. Sir William Crookes devoted a considerable portion of his great scientific talents to practical things, one of these having been a study of possible disinfectants for preventing cattle plague, as a result of which he discovered the value of carbolic acid for destroying germs. R. A. Millikan, the great contemporary physicist, has even derived practical knowledge from his discovery of the cosmic ray, namely, that we can never expect to fall back for our energy supply upon the disintegration of atoms nor upon the building-up of elements, but that we must always, just as in the past, rely upon the sun for our energy.

Sir Humphry Davy, speaking of Benjamin Franklin, suggested concisely the practical place that science

ought to fill in the affairs of men. "Franklin has in no instance," said Sir Humphry, "exhibited that false dignity, by which philosophy is kept aloof from common applications; and he has sought rather to make her a useful inmate and servant in the common habitations of men, than to preserve her merely as an object of adoration in temples and palaces." There is scarcely a bit of knowledge about the world or about man himself that can not, as R. A. Millikan has said, be made to contribute to the finer, fuller, richer, wiser, more satisfying living of the race as a whole. And so it might be a good idea for every scientific explorer to keep an eye open for possible uses of his discoveries. To do so would in some instances reduce somewhat the "purity" of his science, but it is reasonable to suppose on the other hand that it would help to make science—his science—still more useful to people.

Charles F. Kettering is a great believer in making all research useful, if it is possible to do so. As an aid toward that end, he thinks that even in doing research for research's sake, one should do it in such a field and in such a way as will at least offer some possibility of yielding results that will be of service to humanity. If a man is studying dredging, said Mr. Kettering in homely illustration of his view, he might just as well do what dredging he does in a place and along a definite line that could form a usable channel of passage across a too shallow lake, such as Lake St. Clair, as to choose for his experiments a lake altogether re-

mote from navigation and then to dig in a haphazard manner all over it.

Perhaps even more important in its implications than the distinction between pure research and applied is that between two of the forms of applied research. The distinction referred to is whether the research is directed toward the making of some fundamental or outstanding advance in an industry or whether, on the other hand, it is aimed merely at some relatively insignificant improvement in a product—at some mere “gadget” perhaps—or at reducing the cost of making something. Both forms are valuable, of course; but it is an unfortunate fact that industrial research as it has been conducted in some quarters has been too largely, and even exclusively, of the latter and more superficial form. Because the aims of many researches have thus been too low, or because they have consisted too largely of the tinkering form of research, the results have not been as beneficial as they might have been. It is naturally much more important both to an industry, and to society as well, to found a new enterprise or to expand materially the usefulness of an old one than merely to find out how to cheapen the operation of a manufacturing process or how to conduct it with fewer men. The highest form of research, whether its intent classifies it as pure research or applied, is not that which is concerned with things as they are, but that which is aimed clear out beyond existing things to something altogether new.

PART II

METHOD

THE greatest invention of the 19th century," some one has said, "was the invention of the method of invention." It is with this matter of methods of invention, or of the ways in which research is done, that Part II is concerned. No attempt will be made there, however, to outline one specific method of research which is thought to be the best. Willis R. Whitney, one of the greatest of contemporary researchers, says of research that even to-day "relatively little is known about the best way of doing it." So it is that different investigators who hold widely divergent views about just how research ought to be done may yet each of them be quite successful at it.

CHAPTER III

EVOLUTION

FRANK B. JEWETT, head of the Bell Telephone Laboratories, has pointed out that the character of the experimental work which needs to be done by an industrial enterprise sometimes changes with the age of the industry. He has compared evolution in industrial research to the changes that occurred in methods of mining gold in California. In the early days, gold was recovered there by very crude means, by the washing pan, the rocker, and the long tom. Later on, when placer deposits became poorer, mining began to be done by hydraulic methods. Still later, the older methods were supplemented by stamp-mill operations on the richer quartz deposits. As time went on, more elaborate and more accurately controlled methods became imperative. Finally, gold began to be extracted from auriferous rocks by the cyanide process. All these changes necessitated much prior experimentation, and they involved as well the gradual introduction into the gold-mining industry of many kinds of skilled and trained men.

So it is also, as Dr. Jewett said, in some industrial enterprises. To make advances during the early stages of an industry, it is often not necessary to do much

more than to utilize existing stores of knowledge. But soon this accumulation of knowledge gets pretty well picked over. Then there arises a need for new knowledge that is specifically applicable to that particular industry, and consequently for men who can seek out such knowledge.

His own industry, the telephone business, said Dr. Jewett, came to that point about thirty years ago. The consequence has been that under Dr. Jewett's direction the telephone industry has been engaged for many years in a concentrated search for new knowledge that could be of use in the field of communication. This constant search has resulted in the gradual building up of the largest industrial research institution in the country, the Bell Telephone Laboratories. And, incidentally, the results of this far-sighted policy are apparent in the marvelous improvement that has been made in telephone service and in the telephone itself.

The gradual evolution of the kind of experimental work needed for the advancement of an industry is well illustrated also by the development of the gasoline automobile. The motor car is itself the result of an evolution. It is not an invention in the accepted sense. It came into being as an assemblage of a great many prior inventions and discoveries. The ancient experimenter who found out how to kindle a fire—burning his fingers in doing so, perhaps—was possibly the first man who contributed a practical discovery to the motor car. A car with an eight-cylinder engine

running fifty miles an hour touches off ten thousand fires every minute, or six hundred thousand in an hour.

The second great contributor to the automobile was perhaps the inventor of the wheel. Those two things, the wheel that turns and the fire that makes it turn, are the great basic developments upon which all automotive transportation is founded. And the mere fact that no one knows who it was that first tamed fire or who first put the idea of the wheel to practical use does not mean that both those great advances did not come about as a result of intelligent observation and experimentation, and therefore of research of a kind.

A particularly important one of the experimental steps toward the motor car was made about 1840. It was then that Charles Goodyear, experimenting on rubber in a New England kitchen, happened to bring a mixture of sulphur and rubber into contact with the hot stove, and thus discovered how to vulcanize rubber. Other investigators utilized that great discovery of Goodyear's to make a thing of paramount importance to the motor car, the rubber tire. Tires of rubber were applied to the bicycle and to the sulky of the horse racer, and thus gradually developed before the coming of the automobile.

Another real stride toward the motor car was made about 1860 when Lenoir, the Frenchman, built the first successful internal-combustion engine, or engine in which the fire that operated it was made directly within the engine's cylinders. Lenoir was following in

part the example of Christian Huygens, the Dutchman who, using gunpowder as fuel, is said to have built the very first internal-combustion engine just about two hundred and fifty years ago. Lenoir even applied his gas engine to the propulsion of a vehicle.

Then in 1876, Otto, the German, made his great contribution to the internal-combustion engine. This consisted of the important principle of compressing the charge of air and fuel within the working cylinder before igniting it, and it is said to have been the most important event in the history of the internal-combustion engine.

About 1860, also, "Colonel" Drake struck oil in Pennsylvania. In getting kerosene or lamp oil out of petroleum, refiners got as well a large amount of an unwanted liquid, a liquid too light for lamp oil. To-day we call that liquid gasoline. During the 1880's and 90's, a few stationary engines and marine engines were beginning to be run on that by-product, gasoline. It was directly out of these that the automobile engine evolved. Some of them really were the first automobile engines.

It was one of those engines, for instance, that Elwood Haynes put into his first car. He bought it from the Sintz Gas Engine Company of Grand Rapids. It was a one-cylinder upright gasoline engine made for marine use. It was two-cycle, weighed 180 pounds, and delivered only a single horse-power of effort. But in those days it was not customary to hitch more than one horse to a buggy, you know.

Thus, as a consequence of the gradual evolution resulting from spasmodic research along many lines—mechanical, chemical, physical, metallurgical—it was natural that during the years just before and after 1890 several men should have been busy trying to give reality to the age-old dream of a horseless carriage. Just for the sake of old times, it may be said in passing that the names of a few of those men were Duryea, Haynes, Winton, Olds, Ford, Packard, Buick, Daimler, Benz, Levassor, Panhard, and Peugeot. It was due to the persistent and intelligent experimentation of men such as these that the motor car finally emerged from the shop to chug-chug its way along the dirt roads, where it terrified horses with its noise and its unaccustomed smell.

But that early automobile was just a buggy without a horse. It looked like one, too, from its spindly wheels and its buckboard body to the whip socket on its frail dashboard. The greatest question about those early cars was whether they would run or not. So often they would not run that their most useful accessory was a tow rope. When in 1899 the War Department decided to purchase three automobiles for the use of army officers, it was stipulated that suitable provision be made for hitching mules to them for towing purposes. Even an army mule, you see, could be less stubborn and balky than those early cars were at times.

The marvelous advancement which has been made over the crude vehicle of the 1890's did not come about by mysterious nor accidental means at all. It was made

by the intelligent form of experimentation which for short is called research. Every successful motor car manufacturer has had a force of engineers and experimental men of various capabilities constantly striving to improve his product—to make it better and more satisfactory, as well as cheaper. Haphazard and spasmodic invention was thus early superseded by continuous, organized experimentation.

It was thus that the many, many improvements which year by year have been incorporated into cars came into being. In the mechanical field these improvements have consisted of such things as more dependable, more powerful, more economical, lighter, and smoother engines; better means of carburetion, of lubrication, of ignition, of cooling; improved transmissions; easier and safer steering devices; better brakes; better springs and suspensions; and mechanical advances of many, many kinds. We have more beautiful and more comfortable bodies, better and more durable finishes, better and cheaper fuels and lubricants, better and cheaper tires, better steels and useful metal alloys of many kinds.

Improvements in means of fabricating cars have also greatly reduced cost. In the lower-price bracket, the reduction has been from fifty cents per pound to twenty cents, all within the past few years. It was that reduction in cost, or equivalent expansion in the size of the purchaser's dollar, which widened out the market for motor cars to such a phenomenal degree, and which

thereby made automobiles so universally useful. So gradually and so consistently have the improvements in cars come along, however, that it is only by driving a current model in direct comparison with one that is three or four years old that a person is really able to appreciate properly the magnitude of the changes which have been occurring.

Now it should not fail to be said that not all the effort at improving the motor car was expended within the automobile industry itself by any means. A large portion of the improvements came from research done within the materials manufacturing industries, such as the metallurgical industries, the machine tool industry, the abrasive industry, the electrical industry, the chemical industries, the oil industry, and the like.

A list of the chemical and metallurgical materials used directly or indirectly in the automobile industry would include more than 225 different products. Think of all the research which is done by the makers of these many materials, the results of which are passed along in some measure to motor car manufacturers and users. Of the nearly 1600 industrial research laboratories in the United States, as listed by the National Research Council in 1932, about 450, or more than 25 per cent, are either those few within the automobile industry itself or those many which are maintained by suppliers of automotive materials.

Rubber is one of the materials through which research outside the automobile industry proper has made

a large contribution to the motor car. Twenty-five years ago an automobile tire of small size cost about thirty dollars, and the purchaser thought he was lucky if it ran five thousand miles. To-day a tire for a car of the same class costs less than ten dollars, tax included, and it will run fifteen thousand miles or more. Thus it is that, all within the memory of many of us, the cost of automobile tires per mile of driving has been divided by ten.

A story, related by the American chemist, D. H. Killeffer, about how the good solvent butyl alcohol came to be available at just the right time to make possible the modern pyroxylin finishes for cars, is one which illustrates especially well the point about how experimentation and development in widely scattered fields has contributed to the motor car. About 1910, and before, when rubber was very high in price, energetic efforts were being made to synthesize rubber by chemical means. The most suitable starting materials for the rubber synthesis appeared to be the hydrocarbons, called in the parlance of the chemist, butadiene and isoprene. Unfortunately, neither of these basic compounds was to be had easily; but it was known that both of them could be prepared from butyl alcohol.

At this point it was discovered that a special micro-organism or ferment, *clostridium acetobutylicum*, would convert the starch of corn into a product each ten parts of which contained six parts of butyl alcohol,

three parts acetone, and only one part of the customary fermentation product, ethyl or grain alcohol. The growth process of this microbe was accordingly carefully studied; without, however, resulting in any considerable synthesis of rubber.

But in 1914 came the World War with its huge boost in the demand for powder. Now, the specification of the British War Office for smokeless powder required the use of acetone as a solvent. The acetone had always before been obtained as a by-product of the making of charcoal from wood. But now the quantities of powder required were so huge that the wood distillers could not possibly make enough acetone to produce it. In this emergency the new ferment mentioned above, guided by those scientific workers who had learned its habits, came to the rescue. It was put to work in England, in India, and in Canada, changing starch into acetone. The two gallons of butyl alcohol which it made for each one gallon of acetone was, however, largely a useless by-product.

Soon the United States entered the war. And then the President ordered all the whisky distilleries to shut down. But one of those distilleries with an experimental minded manager continued to operate. This it did by changing its ferment from yeast to this new microörganism which made acetone, and incidentally twice as much butyl alcohol, out of corn.

It was thought there that there ought to be some use for all that by-product butyl alcohol. So, instead of

throwing it away, a large vat was built at considerable expense in which to store the by-product butyl alcohol until some use for it could be found. Incidentally, that vat was later converted into a swimming pool. Every one has heard, of course, about the part which the bath tub is understood to have played in another and less legitimate phase of the alcohol business. But it is not likely that many people have heard about the filling of a whole swimming pool up with alcohol. This time, however, it was not grain alcohol but the undrinkable butyl alcohol.

Soon a use was found for all this stored-up butyl alcohol. Research in other quarters had yielded a method of making nitrocellulose solutions of such low viscosity that they could be sprayed on automobile bodies to give a new and better finish. It was in 1923 that this big advance in car finishes was first used. And then both the accumulated stock of butyl alcohol and the method of making it out of corn became extremely important and very valuable, for butyl alcohol is easily converted into that excellent solvent, butyl acetate, which was indispensable to the development of pyroxylin finishes. Thus did butyl alcohol become the primary product of that business and acetone the by-product. Two bushels of corn are said to be required to make enough solvent for the pyroxylin finish which is applied to one car. And, incidentally, here is one of the many ways in which research and the motor car have been of assistance to the farmer.

It is interesting to note in passing that another and apparently even more remote event in which the discovery of this special ferment played an important part was the great Zionist movement by which the Jewish people have been returning to Palestine. The discovery of the method of fermenting corn to acetone and butyl alcohol was made by Professor Chaim Weizmann, a chemist at the University of Manchester, England. So outstanding was the service rendered to England by this discovery, in solving as it did an acute problem in connection with the powder supply, that Lloyd George said to Professor Weizmann:

"You have rendered great service to the State, and I should like to ask the Prime Minister to recommend you to His Majesty for some honor."

"There is nothing I want for myself," replied Professor Weizmann.

"But is there nothing we can do as a recognition of your valuable assistance to the country?"

"Yes, I would like you to do something for my people."

Professor Weizmann then explained to Lloyd George his aspiration to make Palestine a national home for the Jewish people. The result, to make a long story short, was that later on the British Foreign Secretary issued the famous Balfour Declaration, which proclamation became the charter of the Zionist movement.

It is thus as a result of persistent research and ex-

perimentation on the part of many, many men, both within the motor car industry proper and outside of it, that the early horseless carriage has gradually evolved into a vehicle which is at once beautiful, serviceable, durable, and cheap. But, although by comparison with the early automobile the motor car of to-day is a marvelous vehicle, it has not yet reached perfection, of course. Only to the one who is full of complacency or to the man without imagination is anything ever really perfect. It is an old saying that no tree ever quite reaches heaven.

For several years following the birth of the motor car, progress in the industry was made for the most part simply by adapting or by putting into practice information already available, or at least by using information which could be got without doing much in the way of pioneering research. But most of the easy things have already been done. And so, in order to keep on making the automobile constantly better and better, the automobile industry itself, and also those many industries which serve as supplies of materials to it, have gradually had to turn more and more to the pioneering form of research, or to what really amounts in some instances to digging down into the fields of the fundamental sciences.

To suggest in a more concrete way how the pioneering form of research has now become indispensable to the advancement of the automobile industry, it is only necessary to name a few specific instances of the

further advances needed. Thus in metallurgy one of the outstanding needs is for cheaper light metals. It is a striking fact that there is several times as much rubber as aluminum in the average car. The reason why aluminum is not used more extensively in making cars is not that its potential usefulness is small, but that its cost is still too high.

Now the problem involved in lowering the cost of aluminum is the fundamental one of finding out how better to pry the metal loose from oxygen and silicon in its natural compounds or ores. The supply of aluminum ores is no problem. Aluminum is more plentiful than iron. It is, in fact, the third most abundant element in the world. But so tightly held in its compounds is aluminum that no one has yet found a cheap method of isolating the metal. Here is a problem which demands the real pioneering form of research. Also worthy of consideration in this same connection are the immense possibilities which appear to be latent in the still lighter metal, magnesium, and in that other light metal of unusual properties, beryllium.

Even in the case of iron and steel there is still much of a fundamental nature that needs to be known about their metallurgy. One thing that is particularly needed is more perfect means for the rust-proofing of iron. The advantages to be gained by finding out how to keep oxygen from attacking iron, and of doing so cheaply, are simply beyond computation.

And then there is the important matter of lubri-

cating cars. A better understanding is needed of the fundamental basis of lubrication. As yet it is not even known why one liquid is a good lubricant, while another apparently similar one is not a lubricant at all. In a practical way, the automobile industry is in need just now of lubricants which will permit higher pressures to be applied to bearing surfaces. But the search for such a substance is handicapped by lack of knowledge of just how a lubricant really does its work.

Badly needed also is a better knowledge of what happens within the combustion chambers of the automobile engine when gasoline and air burn there. Very little is yet known about what really happens when gasoline burns in an automobile engine. And this is so in spite of the fact that the whole automobile industry, and all automotive transportation in fact, depends upon that event.

We need still better and lighter engines, more perfect transmissions, better brakes, better springs, still better finishes, and the like. We need to know more about the strains set up in metals by heat. We ought to know how to run engines on leaner fuel mixtures, and so at once to improve economy and cure the stinking of engine exhausts which some one has dubbed "highway halitosis."

These are just a few instances from the many pertaining to the motor car which suggest why it is that those who now are trying to improve automotive transportation still further have come to consist not only,

as at first, of conventional engineers. It is why they now include also men capable of conducting fundamental and pioneering researches in such sciences as chemistry, physics, and mathematics—and even botany and bacteriology. “The successful automobile body designer of the future,” said H. G. Weaver to the Society of Automotive Engineers recently, “must go beyond his engineering handbooks and be up on such things as physiology, psychology and neurology.” But, of course, the work of these pioneering investigators is naturally supplementary to that of conventional engineers and practical experimental men. For such men there still is, and no doubt always will be, a place of prime importance in the automobile industry—and, of course, in other industries as well.

CHAPTER IV

ORGANIZATION

SIR FRANCIS BACON, in his fable *The New Atlantis*, described an institution which he called Solomon's House. Now Bacon did not mean the kind of Solomon's House that the name may first bring to one's mind. It was not the king's palace inhabited by those one thousand quarrelsome wives of his, and visited one time by the Queen of Sheba. It was not the palace of Queen Balkis and the butterfly that stamped, which Kipling wrote about. It was really an organized research laboratory, and a highly organized one at that. Bacon said of his imaginary Solomon's House that it was "the noblest foundation...that ever was upon the earth."

It was perhaps because of Solomon's reputation for wisdom that Bacon called this fabled institution of The New Atlantis by Solomon's name. The wisdom implied here is the wisdom of establishing such a forward-looking institution, and not, it should be said, any peculiar sagacity on the part of the individual workers in it. "The end of our foundation," said the father of Solomon's House, "is the knowledge of causes, and secret motions of things; and the enlarging of the bounds of human empire, to the effecting of all

things possible." How is that as a program for a research laboratory?

Solomon's House was the most useful and highly respected of all the institutions in The New Atlantis. It was the lantern (lanthorn) of the kingdom. Its workers were organized into nine separate sections or divisions, and they conducted experimental researches in many fields. A few of these researches were those on how to maintain health and to cure diseases; on foods; on fabrics; on dyes; on radiation; on means of making light and of separating light into "single" colors; on telescopes and microscopes; on engines; on sound, on controlling its quality, and on transmitting it over long distances; and even on airplanes, on submarines, and on imitating the "motions of living creatures by images" (moving pictures?).

All of this, and more, Bacon imagined about the year 1623. And so, although the organized research laboratory as we know it is comparatively a new thing, the idea is three hundred years old. Incidentally, all of the scientific research of Solomon's House was aimed directly at practical utility or service to the people of New Atlantis. Thus the research that Bacon imagined was applied research.

Nothing that approximated the applied research laboratory pictured by Bacon existed until quite recent years. Nearly all the organized research laboratories, at least in the United States, came into being within the current century. It was in 1900 that Willis R. Whit-

ney began research in a corner of one of the buildings of the General Electric Company in Schenectady. The United States Bureau of Standards was not constituted on its present basis, under which organized research is a part of its activities, until 1901. And it was a few years after that before the United States Bureau of Mines began doing organized research. It was about 1902, that Charles L. Reese organized the Eastern Laboratory, pioneer among the du Pont Company's several research laboratories. Frank B. Jewett, head of the Bell Telephone Laboratories, started on his career of research in the telephone business in 1904. The Research Laboratory of the Eastman Kodak Company was established under C. E. K. Mees in 1913. Organized research as a separate activity was begun by the Westinghouse Electric and Manufacturing Company in 1917. The central Research Laboratory of General Motors Corporation, now headed by Charles F. Kettering, was established in 1910, although each one of the various divisions of the corporation has conducted research independently from the time it began making cars.

Before 1900 it was nearly altogether by detached and lone-handed experimenters that scientific and practical advances were made. The Curies discovered radium after a long period of unassisted toil. The telegraph was practically the lone-handed invention of the artist, S. F. B. Morse; and the telephone of the teacher of speech to deaf-mutes, Alexander Graham Bell.

Michael Faraday was the unaided Columbus who discovered the important principle of electromagnetic induction, and who spent long years in trying to "change magnetism into electricity," or in common parlance to make an electric generator. The locomotive, the steamboat, and the automobile, all were gradually developed into practicalities as a result of the accumulated contributions of many detached experimenters.

Nearly every one of the early experimenters labored under great difficulties, and many of them had to work with the most inadequate of facilities. Sometimes it was truly a case of what Herbert Hoover called "genius in the woodshed." The Curies made their long search for radium in a tumble-down wooden shed that had been a dissecting room. It was in a Salem cellar and in a Boston attic that Alexander Graham Bell toiled toward success with the telephone. Having little or no help, and usually no money to pay help with even when it could have been had, each experimenter generally built with his own hands all his apparatus and instruments. During his experiments on the telegraph, S. F. B. Morse even had to make every foot of the insulated wire he used, which he did by the slow and laborious method of winding cotton around bare wire. At one time while trying to make his electric motor, Thomas Davenport tore his wife's silk wedding dress into strips for insulating the coils of wire. Antony Leeuwenhoek made both the lenses and the microscopes with which he peered into the sub-visible world, and

William Herschel personally constructed the telescopes by means of which he scanned the heavens. Once, while Herschel was finishing a seven-foot mirror for one of his big telescopes, he had to keep polishing steadily for sixteen hours at a stretch, without taking his hands off the mirror. The Wright Brothers had to do more than learn how to build a flying machine. They had as well to develop and build an engine that was at once light enough and powerful enough to fly it with. Goodyear made some of his experiments on the vulcanization of rubber while confined in a Philadelphia jail as a common debtor.

Not only did most of the early experimenters have no organization to help them and to offset their shortcomings, but many of them experimented only during such time as could be spared from the necessary toil at which they made their living. But the fact that some of these were thus *spare-time* workers should not be taken to mean that, as judged by present-day standards for hours of labor, they were *part-time* workers at all. Usually far from it. To the lone-handed and money-shy investigator, even sleep had to be quite a secondary consideration.

In one important respect the detached or single-handed investigator labors under an even greater handicap to-day than ever before. This is particularly true if he attempts to do research that applies to any industry already established. As a result of the evolution already mentioned, things have now become so com-

plex that any worth-while improvement in them usually demands not the unbalanced effort of the lone-handed investigator but the systematic endeavors of a well-rounded-out organization. To-day it generally happens that the one-man product is too limited or lopsided either to fit into any established system or to replace it.

That is why in research, and particularly in applied research, there has been a change from solo to ensemble. "The essence of modernity," says W. E. Wickenden, "is that progress no longer waits on genius—instead we have learned to put our faith in the organized efforts of ordinary men."

Although organized research has come into being almost altogether since 1900, there is to-day perhaps not one forward-looking industry that does not maintain some form of organized experimentation or research. Bulletin Number 91 of the National Research Council, issued in 1933, shows that in 1932 there were about sixteen hundred industrial research laboratories in the United States. The reason why there are now so many industrial research laboratories in existence is that industry has found out that depending for the improvement of its products and its processes upon the chance contributions of outside genius is not a policy that enables a business to advance or to secure its future.

As classified by C. E. K. Mees, there are seven principal types of organized research laboratories in exist-

ence to-day. These, with illustrations of each added, are as follows:

1. *University Laboratories*.—Nearly every American university.

2. *Government Research Laboratories*.—Bureau of Standards, Bureau of Mines, Bureau of Chemistry of the Department of Agriculture, Forest Products Laboratory.

3. *Foundation Research Laboratories*.—Rockefeller Institute for Medical Research, Bartol Research Foundation of the Franklin Institute.

4. *Industrial Research Laboratories Maintained by Individual Firms*.—The du Pont Company, American Telephone and Telegraph Company, Eastman Kodak Company, General Electric Company, General Motors Corporation.

5. *Coöperative Research Laboratories*.—National Canners' Association, Portland Cement Association, Tanners' Council of America, American Institute of Baking.

6. *Industrial Fellowship Laboratories*.—Mellon Institute of Industrial Research, Battelle Memorial Institute.

7. *Private Consulting Research Laboratories*.—Arthur D. Little, Inc., The Miner Laboratories, Thomas and Hochwalt Laboratories.

It is only those institutions which would be classified under numbers 4, 5, 6, and 7 above that make up the list of nearly sixteen hundred industrial research laboratories mentioned earlier.

The basis on which a research laboratory, and particularly an industrial research laboratory, is organized is much the same as that of a business of any kind. In the words of Frank B. Jewett, a research laboratory "represents the same kind of organization as has been

found necessary for the proper functioning of any of the other major parts or divisions of industry." There is, first of all, a director or responsible head; and functioning under him are such assistants and supervisors as are needed, together with a staff of men having the various capabilities required for the systematic prosecution of the job or jobs in hand. This usually means that the staff is made up of men selected from some of the following groups: chemists, physicists, bacteriologists, mathematicians, engineers, draughtsmen, instrument makers, mechanics, along with testers, assistants, and helpers of various kinds.

There are three major systems under which research laboratories have been organized. The first is the departmental. In this, as in business in general, the activities of the laboratory are systematically classified into definite departments with a suitable man in charge of each. The second has been called the cell system. In this type the organization consists of a number of investigators of equal standing, each directly responsible to the director of the laboratory. The third is a combination of the departmental and the cell systems, in which the cell system may prevail within some of the departments, with each investigator reporting to the head of his department. Which system of organization is best in any given instance is determined by the specific conditions prevailing there, such as the character and magnitude of the problems being investigated, the kind of men working on them,

and whether the problems under investigation are all on one general subject or whether they are of different kinds. The natural limitations and the unavoidable one-sidedness of any one man make it important that in the cell system the cells should not be so tight that ideas from other parts of the laboratory can not get into them.

Again, with respect to the way in which research is conducted there, research laboratories may be divided into two types: the *divergent* and the *convergent*. Under what is thought of as the divergent system, the various men working on a project are entirely dominated by the head or the leader. Everything there radiates out from him. He it is who furnishes nearly all the ideas and who even decides upon the specific means by which they shall be tested or put into effect. This is in substance the principle of the old-time individual inventor, but pursued now in an organized way or under conditions which provide him with plenty of help to carry out his directions.

In what is thought of as the convergent system, on the other hand, the staff is made up of a group of men of large individual capabilities or attainments of their own, all of whom pool their ideas and efforts towards some specific end. The principal radiation now is from the outside toward the center; although, as a usual thing, the radiation is in both directions, instead of being altogether from the center outward as in the divergent system. In the convergent system the final

result thus embodies the ideas, contributions, or inventions of several men working in a coöperative manner.

The two systems of research just named are paralleled precisely in business. "There are two kinds of big men in business," to quote one writer. "First there is the business builder" who "rarely surrounds himself with able associates. . . . He wants men around him who do what he tells them to do—pronto. . . . His ideal of a perfect prime minister is an echo. . . . The other kind of big man in business builds and leads an organization of men—the men build the business."

No one man can usually do very much in any endeavor. It is only as he is able to bend the efforts of several other men together with his own into the groove which he is following that he is able to accomplish a great deal. In research it is usually more effective then for a leader to get the efforts of independently capable men to flowing through some mutual channel (convergent research), than for him to bend the efforts of those who are simply blind assistants or mere helpers into a groove traced and fed altogether by himself alone (divergent research). "No large research laboratory," said W. D. Coolidge, "can be successful on the basis of a director and a group of assistants. The number of mere assistants, in fact, which can be used to advantage is much smaller than one might think."

Because research men are sometimes temperamental in type, the best organization for a research laboratory

is often that in which there is only the necessary minimum of formal organization. Research is creative work, the quantity and quality of which is dependent to a large degree upon the enthusiastic coöperation of certain men or groups of men; and hearty coöperation is sometimes best secured where the organization is not so rigid as to hamper freedom of individual effort. C. E. K. Mees has said that the ideal organization of research consists in "getting good research men and letting them do what they want to." But this ideal, he says further, cannot actually be attained, because all research men are not equally good and because of the necessity of some formal system in every institution of any considerable size. Frank B. Jewett has said that the picture which some like to paint of the ideal research laboratory "as a place provided by industry in which trained scientists are free to do whatever they please on any kind of a problem that happens to strike their fancy . . . is a foolish picture, and one likely to do great harm."

The first problem in organizing an industrial research laboratory is how to go about it. One way not to do it, in the mutual opinion of Charles F. Kettering of General Motors and Charles E. Skinner of Westinghouse, is to put up a building and then go out and hire a group of research men to fill it. On the basis of their considerable experience in doing and directing industrial research, they believe that the best way to build up an effective research organization is to start

out in quite a modest way, maybe with only one or two or three men; to let those men begin by seeking for a solution of some obvious problem of the business; and then to expand the activities gradually, as definite need for expansion develops and as men of proper qualifications become available. Such a method is equally applicable to the large business and to the small. Whether ultimately large or small, the best research laboratory does not come into existence full grown, as the goddess Minerva is said to have sprung from the aching head of her father, Jupiter. The business man and the research man both have their headaches to-day, but they are not such as to give birth all at once to a full grown research laboratory. It is better for such an organization to be born as a baby and to grow up to functional maturity in a normal manner.

The advantages of organization in research are shared alike by the institution that maintains the research and by the men who do the research. The institution, if it is a manufacturing one, for instance, benefits in such ways as by the stabilization of its business, by the improvement of its products, by the finding of new products, by the cheapening of its processes, by finding uses for its by-products, and by the opening up of new fields for its products. The individual investigator benefits by being able to do the kind of work which he likes and for which he is best fitted, and under conditions so favorable as never to have

been dreamed of by the detached investigators to whom we owe the advances made in previous generations.

In organized research, the investigator has a pleasant place to work, he has the physical facilities and assistance that are necessary, he does not usually have to worry about where the money is to come from to support either his work or his family, and he has a reasonable assurance that the results of his work if successful will be put to use. If, on the other hand, his particular work should not yield the results that are hoped for, as some researches must necessarily fail to do, he is not completely ruined by it. The chances are that other work in the same laboratory will be successful enough that his failure will not be a serious blow to the institution for which he works; so that he can begin research on another problem with the hope of better luck next time.

Karl T. Compton has said:

It is certain that a considerable portion of experiments which are well worth trying will prove to be unsuccessful, whereas others will be successful and some few will be really great contributions. This is well understood by the directors of great industrial research laboratories.... They know that much of the work which is done will turn out to be unprofitable, but they realize that it is worth the effort because, out of the whole group of researches, if intelligently carried on, there will be some so successful as to more than justify the entire effort. This is not always realized by the industrialist who has no background in research, who hears

research being talked of, and decides that he will try it and then quits in disgust if his first attempt proves unsuccessful.

After the idea of the Rockefeller Institute for Medical Research had been conceived, John D. Rockefeller, Jr., said to his father, "Here's a great gamble. You may plant one million or five millions and get no crop in the form of medical discovery. The average man cannot afford to put in several million dollars without knowing definitely that there will be tangible results. You can." And luckily he did. It is this matter of the uncertainty of the success of any single research endeavor which caused Sir J. J. Thomson to say, "The researcher, if he is to have a happy life, must regard the game and not the score as the chief thing."

If a research project is years in reaching completion, which very often happens, the worker at organized research and his family do not have to starve through the weary months, as has been the fate of many detached investigators. If the financial reward of the worker is not as great as that ultimately secured by the more fortunate of the individual experimenters, it is at least better than the average reward of such investigators, for the great majority of them have lived and died in the poverty produced by years of self-supported experimentation. When, for instance, Charles Goodyear died, after having shown the world how to vulcanize rubber, he was more than poor—he was in debt to the amount of two hundred thousand dollars.

John Fitch, discouraged and poverty-stricken after all his experiments on the steamboat, took his own life. Mrs. Carrie Everson, who discovered the valuable flotation process for ores, never made a penny of profit from it. LeBlanc showed the world how to make cheap alkali, but died in a French poorhouse. There are around two million American patents, but, as Willis R. Whitney has said, "not one per cent of the hard-working inventors were ever rewarded at all."

The value of organized research is that it does away with the evils of haphazard experimentation. It substitutes system for chance. In so doing it becomes at once an aid to industry and a help to the research worker, not to mention at all the benefits that it confers upon society.

CHAPTER V

LABORATORIES

IT is not really in a laboratory that problems are solved. Instead, as Charles F. Kettering has said, they "are solved in some fellow's head.... All the apparatus is for is to get his head turned around so that he can see the thing right." Once that has been done, the answer presents itself—which it usually does quite unexpectedly—and then the problem is solved.

It was while in his bath that Archimedes is said to have made the historic discovery that whenever a body is immersed in a fluid it is buoyed up with a force just equal to the weight of the fluid displaced. H. von Helmholtz said on his seventieth birthday that, after he had been working on some problem for a while, "happy ideas" about the solution of it usually came to him unexpectedly like an inspiration. "So far as I am concerned," said Helmholtz, "they have never come to me ... when I was at my working table."

The aria of the beautiful quartet in the "Magic Flute" is said to have popped into the mind of Mozart while he was playing a game of billiards. Mozart always carried with him a note-book against just such occasions. It was while riding atop a London bus one night that Professor Kekule saw the atoms dance about and arrange

themselves into his famous "benzene ring." S. F. B. Morse conceived the telegraph on board "the good ship *Sully*" while returning from a trip to Europe. And it was while toiling on foot up to the Furka pass in the mountains of Switzerland, far away from his basement laboratory at Columbia University, that Michael Pupin invented the Pupin coil which has since done so much for long-distance telephony.

Telling of his researches which resulted in the discovery of the process of half-tone photo-engraving, Frederic E. Ives said, "I went to bed one night in a state of brain fag over this problem and the instant that I awoke in the morning saw, before me, apparently projected upon the ceiling, the completely worked out process and equipment in operation." Charles Darwin, relating the circumstances under which the formulation of his great theory of evolution came to him, said, "I can remember the very spot in the road, whilst in my carriage, when to my joy, the solution occurred to me."

Here is James Watt's own story of how he made his historic invention of the condensing steam engine:

I had gone to take a walk on a fine Sabbath afternoon. I had entered the Green and passed the old washing house. I was thinking of the engine at the time. I had gone as far as the herd's house when the idea came into my mind that as steam was an elastic body it would rush into a vacuum, and if a connection were made between the cylinder and an exhausting vessel it would rush into and might there be

condensed without cooling the cylinder. I then saw that I must get rid of the condensed steam and injection water if I used a jet, as in Newcomen's engine. Two ways of doing this occurred to me: First, the water might be run off by a descending pipe, if an offlet could be got at the depth of 35 or 36 feet, and any air might be extracted by a small pump. The second was, to make the pump large enough to extract both water and air....I had not walked farther than the Golfhouse, when the whole thing was arranged in my mind.

Thus, from all these instances, it appears that the product of the subconscious mind—or, as Oliver Wendell Holmes said, “of work done in the underground workshop of thought”—sometimes has a large place in research. Unconscious reasoning even appears to be better at times than conscious reasoning. Might it not well be that a greater knowledge of the mechanism of these unconscious functions, or of how to control or to make more positive and effective use of what are now pretty largely unconscious mental processes, would be a big boon to the investigator?

But it should not be inferred from the above that great discoveries have a habit of popping unbidden into the minds of certain chosen people. Rarely indeed does such a guest come to any one unless he has first passed through a period of intensive preparation, and usually the period has to be a long one. This preparation may consist of reading, of study, of calculation, and—most important of all—of laboratory experimentation. What a large amount of these preparatory or educational en-

deavors must have preceded the bursting of the illuminating ideas upon those experimenters mentioned above: Archimedes, Helmholtz, Kekule, Morse, Pupin, Ives, Darwin, and Watt. The effect of the various courses in the school of preparation is to educate the experimenter on the problem on hand; or, as was said in the opening paragraph, "to get his head turned around so that he can see the thing right." A research laboratory, then, is essentially an educational institution—it is an institution for the education of the research worker himself.

Now an educational institution may have either an elaborate equipment or a most rudimentary one, as may also a research laboratory. The character of the education received in a school does not necessarily depend upon the kind of equipment it provides. More than anything else, it depends upon the teachers and the kind of men who receive their education there. "My definition of a University," tradition has James A. Garfield as having said, "is Mark Hopkins at one end of a log and a student on the other." If elaborateness of apparatus is a mark of a good laboratory or of a successful research worker, then Joe Cook should be a scientific genius, instead of just a comedian.

Sir Richard Gregory has told the following story about the great English chemist and physicist, William Wollaston, who was president of the Royal Society in 1820. One day a visitor came and asked Dr. Wollaston whether he might see his laboratory. "Certainly," re-

plied Wollaston, and rang a bell. "John," he said to the attendant who answered, "bring up my laboratory." In a few minutes John returned with all of Wollaston's experimental apparatus on a tea-tray.

Robert Hare, a Philadelphia chemist born in 1781, was credited by Edgar Fahs Smith with having constructed the first electrical furnace. Not only must that furnace of Hare's have been a poor one by present-day standards but also he had no more electromotive force to operate it with than could be got from a voltaic pile. Yet it was with such an outfit that Hare converted charcoal into graphite, volatilized phosphorus from its compounds, isolated metallic calcium, and made calcium carbide. When James Watt in his inadequate shop came to build a model of the condensing steam engine which he invented during his walk on the momentous Sunday afternoon, he had to make the steam cylinder and the piston out of a surgeon's large brass syringe.

That day in 1886, when in the woodshed at the rear of his parents' home in Oberlin the youthful Charles Martin Hall found out how to make metallic aluminum from bauxite by electrolysis, he had no other apparatus than a few small crucibles, a home-made furnace and bellows to heat his crucibles with, and a galvanic battery to furnish the electricity. When Pasteur went off to Arbois to study the diseases of wine in the effort to save the then imperiled industry of his home town, he set up his laboratory in an old room that had

been a café. Such crude apparatus as he had was made by the carpenter and the tinsmith of the village. Instead of a gas burner he had to use an open charcoal brazier, which, when occasion demanded, his assistant Duclaux made to glow with a pair of bellows. There was no convenient sink and running water in that laboratory. Whatever water was used there Duclaux carried from the town pump. There were in fact none of the conveniences of the modern chemical or bacteriological laboratory. Yet it was there in that crude laboratory that pasteurization was discovered, and incidentally that the great wine industry of eastern France was saved from going bitter and ropy and oily.

In spite of all that has just been said, however, excellent laboratories are a great aid to research. Good workmen not only deserve good tools, but also, if their best work is to be done, they must have good tools. A first class woodsman can fell a tree more quickly with a dull ax than a poor woodsman can do it with a sharp ax. But the largest number of trees are felled when the sharp ax, and a saw as well, are put into the hands of the good woodsman.

Laboratory experimentation, as has been said, is merely an educational process. And naturally a better and quicker education can be got where there are facilities for building good apparatus and models, where there are plenty of accurate measuring instruments, and where experiments can be carried on in an organized and uninterrupted way. Clean, light, well organized,

and adequately equipped laboratories foster two very important things: excellence of work and happiness of the men doing the work.

Another thing, pointed out by C. E. K. Mees in one of his addresses, is that research which is properly organized and equipped makes it less necessary to depend upon outstanding genius for improvements. The careful, painstaking collection of pertinent facts, for which the properly organized research laboratory is fitted, gives information that people of usual capabilities can interpret. So it is that money spent in building and equipping a good research laboratory is money well spent.

And many excellent research laboratory buildings have been constructed, particularly during the past ten or fifteen years. Many of these have been in industry, such as the laboratories of the Aluminum Company of America, the A. O. Smith Corporation, Standard Oil Development Company, the Eastman Kodak Company, and General Motors Corporation. Some have been in colleges and universities, such as Princeton University, the Ohio State University, Cornell University, Purdue University, and the University of Michigan. Others have been built for the various kinds of research institutions listed in the earlier discussion of Organization.

But naturally these organizations know that the mere possession of a fine, completely equipped laboratory is of itself no sign that great results will be secured there. For, after all, the really important thing in research,

just as in any endeavor, is the kind of men who are making the searches, and the earnestness and persistence with which they pursue them. "I would sooner have one good, thinking man in an attic," Charles F. Kettering has said, "than all the big laboratories and equipment in the world." The institution which depends upon laboratory facilities alone is leaning on a broken reed. It is somewhat like what Arthur D. Little said about an organization chart: "There is danger in an organization chart—danger that it may be mistaken for an organization." Of the two things, a laboratory and the staff which man the laboratory, the staff is a great deal the harder to build. It also takes much more time to build up a good research staff than to build a laboratory.

As for the research worker himself, there are some things that no amount of organization nor excellence of facilities can do for him. Nothing can do away with the need on his part for self-reliance, for head work, and for hard work.

CHAPTER VI

SENSES SUPPLEMENTED

DON'T you wish you had the nose of the dog; the eye of the eagle; the ear of the antelope?" The answer to this question of William Lyon Phelps, so far as the research worker is concerned, is that, even if he had all three of them, it would still not be enough. One of the biggest problems in research happens to be that of supplementing the various physical senses of the investigator as aids to his powers of observation. The development of instrumentation it is called, and every research laboratory has to do a great deal of it. Through the aid of sense-supplementing instruments, long strides have been made in extending the scopes of the normal human senses.

Of the five senses which convey to the mind of an investigator impressions of things outside his body, three—touch, hearing, and sight—are physical; and two—taste and smell—are chemical. Touch tells an experimenter of pressures, of temperatures, of the texture of surfaces. Hearing tells him about the character of vibrations in the air, and sight about waves in the ether.

But the two wave-sensing senses are good only over a small portion of the range that waves cover. If the

research worker needs to know about a vibration of any particular frequency, the chances that it will lie within the range of his unaided hearing or of his un-supplemented sight are really not very great. Such radiations spread over about sixty octaves. Of those sixty octaves, we can hear only ten and see only one. Outside of these narrow bands we are altogether deaf and completely blind. The X-rays that are used to see through opaque bodies, for instance, are nine octaves beyond the range of our unaided sight. And the cosmic rays that Millikan and Cameron have found to be everywhere around us are twenty octaves further still out beyond any of the human senses.

The range of the human eye has been extended a million times by such instruments as the telescope, the microscope together with the ultra-microscope, the spectroscope, the stroboscope, the fluoroscope, and like aids to seeing where the eye alone can not see. To aid further the vision of his powerful microscope, the bacteriologist stains with different colored dyes the bacilli he is searching for, and the metallurgist first etches between the crystals of the steel he is examining and then makes a micro-photograph, or a many-fold enlarged picture of it, which he can then carefully examine at his leisure. These instruments make human eyes a thousand times better than those of the eagle. They make it possible to examine objects no larger than the one-millionth of an inch in diameter, to see out into space at objects billions upon billions of miles

away, to look at rapidly moving objects just as if they were standing still, or to see right through opaque bodies.

To make his ear much better than that of the antelope even, the research worker develops a device for the many-fold amplification of sound, or one for the analysis of it into its individual components. He may even convert sound into some other form of energy, such as electrical, which he happens to have been able to make instruments for analyzing. In radio and the telephone such conversion of sound is done as a regular thing. With proper instrumentation the experimenter can hear noises that are infinitely faint, he can listen to sounds originating thousands of miles away, or he can hear vibrations the frequencies of which are away beyond the scope of his unaided ear. Short radio waves, for instance, are in pitch ten octaves or so above the limit of the ear. And besides they are not vibrations in the air but in the ether.

To improve his sense of touch or feeling, the scientific explorer makes a thermometer to tell him how cold things are, a thermopile to tell him how much heat is being radiated by a body, a balance or a scale to tell him how heavy things are, or a scleroscope to tell him how hard surfaces are. Sir William Ramsay, in his research which led to the discovery of five new gases in the atmosphere—helium, neon, argon, krypton and xenon—built a balance so sensitive that it could detect a difference in weight of the one-one hundredth mil-

lionth of a gram, which is only the one-forty-five billionth part of a pound. For finding out how much heat reaches the earth from the stars, instruments have been made that are sensitive enough to detect differences in temperature of the one-ten millionth part of a degree centigrade. With such sensitive means available for evaluating it, amounts of heat have been measured which are no greater than that coming from a single candle located six miles distant.

The chemical senses of smell and taste tell about the chemical make-up of things outside the body. But so imperfect are these senses of themselves that as a usual thing their usefulness in research is not very great. In research, information about chemical composition must usually be obtained not directly through either one of the chemical senses but by indirect chemical and physical methods.

Because his sense of taste is not nearly delicate enough, the research worker develops chemical and electrical means of measuring minute amounts of acidity in terms of *pH* values, or he uses a chemical color indicator, like litmus, to tell him whether a liquid is acid or alkaline; he makes a saccharimeter to tell him how sweet a liquid is—or rather how much sugar it contains. In the same way he makes a supplementary nose of some kind by which he can detect minute traces of substances in the air, and even of substances like carbon monoxide which of themselves have no smell.

Automatic smellers for the carbon monoxide produced by motor car engines have thus been used in such places as the Liberty Tunnel at Pittsburgh and the Holland Tunnel at New York. These devices do not smell carbon monoxide as the human nose might, for so far as the nose can tell carbon monoxide has no odor. What they do instead is to burn the gas by breathing it, along with air, over an active combustion catalyst called hopcalite—one of the substances used in the doughboy's gas mask—and then to record the temperature generated by the burning. So sensitive to carbon monoxide are these supplementary noses that they can detect as little as two parts of it in a million parts of air. And, if the concentration of carbon monoxide should ever happen to rise high enough to make the air dangerous to breathe, then they ring a warning bell.

That instruments are one of the first needs in research is thus not alone because the natural human senses are not sensitive enough. It is also because they are merely qualitative and not quantitative. By tasting an acid liquid an investigator can tell that it is sour—provided, of course, that it contains enough acidity for him to taste—but he can not tell just how sour it is. He can hear a sound, provided that it is loud enough and that it comes within the narrow range of his ear, but he can not tell what its pitch is. He can see that a tree is tall or that a liquid is red, but he can not tell just how tall the tree is nor how red the liquid is. The

same lack of quantitateness applies also to the other two senses, smell and feeling. But in research things must be known with exactitude. And so it is that instruments have to be developed to supplement the physical senses of the experimenter with the essential element of quantitateness.

By virtue of having introduced the balance as an instrument of quantitateness into the study of chemical processes, Lavoisier became one of the founders of modern chemistry. "The pivoted lever that forms the analytical balance of the chemist," said Harvey Brace Lemon, "probably is the most important single tool by means of which curious human beings have ever pried into the Pandora box of nature."

It was as a means of getting an invariant and always reproducible measure of length that Professor A. A. Michelson developed the instrument called the interferometer or the inferential refractometer. The interferometer measures lengths in terms of wave-lengths of light. The length of the master measuring stick of the world, the standard meter at the Paris International Bureau of Weights and Measures, of which we have a duplicate at the U. S. Bureau of Standards, has thereby been obtained in terms of an eternal, invariable measure, the wave-length of the red light of cadmium. The colorimeter was developed as an aid to the eye in matching colors exactly, and the photometer in the same way as an aid to the precise matching of light intensities. In order to find out just what engines and

automobiles will do in the way of performance, use is made of such instruments as the dynamometer for getting an exact measure of power output, as the accelerometer for measuring rate of acceleration, and as the fifth-wheel speedometer for the precise measurement of speed.

Great advances in old arts or sciences come sometimes from the ability given by some newly-developed instrument for the observation or measurement of old things in new or more refined ways. Such an advance in instrumentation need not come from within the industry to which it turns out to be useful, of course—that is to say, it need not if the industry is an alert one. Astronomy is an instance of an old science which has seized upon the various developments made from time to time in sister-sciences, such as in physics and chemistry, and used them to help advance knowledge in astronomy. One of the first of these instruments to be adapted was the spectroscope. The spectroscope gave the astronomer important information about chemical constitution, about velocities of movement, about pressures in stellar atmospheres, and about the temperatures of the stars. Other adapted instruments of great value to the astronomer have been the photometer, the thermopile, the bolometer, and the radiometer. The photo-electric cell also has given photometric measurements on stars of the highest precision. The interferometer as well has served the astronomer for many purposes, notably for accurately measuring the diam-

eters of giant stars. And just recently the motion picture camera has also been impressed by astronomers as an instrument of observation.

Thanks chiefly to these and other advances in instrumentation made pretty largely outside the field of astronomy itself, the precision of astronomical measurements in general has been greatly improved in recent years. Thus, as George Ellery Hale has pointed out, the Greeks gave star places only to the nearest ten minutes of arc, which is such a large degree of variation as to be equivalent to one-third the diameter of the moon. Tycho Brahe, working during the last half of the sixteenth century with instrumentation at his disposal then, succeeded in reducing the probable error of a single measurement of the distance between two stars by ten times, or to slightly less than sixty seconds of arc. But the modern interferometer method of measurement is sixty thousand times as accurate as the measurements of Tycho Brahe, or six hundred thousand times as precise as those of the Greeks. And the major portion of this improvement in accuracy of observation came about not through instruments developed especially for the purpose, but through the alertness of astronomers in seizing upon and utilizing instrumental developments made in other fields of science.

What the astronomer has thus done establishes a precedent that is applicable also in some measure to research in every art and every science. And it is a precedent that is being largely followed. Thus, for instance,

animal and plant physiologists use in their researches the delicate D'Arsonval galvanometer of the physicist; pathologists in studying heart action make use of vibration galvanometers; diseases of the ear are investigated with the aid of vacuum tubes and the technique of the radio engineer; and the biologist studies cell activities in terms of osmosis and surface tension. These and other representative instances of such adaption of instrumentation have been cited by Harvey Brace Lemon of the University of Chicago.

If it is to be successful, the research laboratory has to be well supplied with instruments for supplementing the various human senses, and for doing so in a quantitative manner. Some of these instruments are the conventional ones used in many places, such, for instance, as the long list of meters: galvanometers, ammeters, wattmeters, hydrometers, thermometers, pyrometers, photometers, radiometers, bolometers, spectrometers, refractometers, and so on. Others are special instruments developed for some peculiar purpose, which at one time or another every explorer in a new field is sure either to have to provide for himself or to adapt from some other investigator. And it may even happen, as indeed it sometimes has, that the degree of success an investigator meets with will depend upon his ingenuity at developing, or adapting from others, precise means of supplementing his imperfect physical senses.

CHAPTER VII

PAPER EXPLORATION

WHEN Thomas A. Edison was a boy, he set out to read the whole Detroit Public Library through. Needless to say, he did not realize that ambition. And perhaps it is just as well, for it would certainly have given him literary dyspepsia. But, fortunately for Edison and for the rest of us, literary dyspepsia is a thing that he never did get. Throughout his long life Edison read everything he could get hold of on subjects that he happened to be working on, as well as a great deal on matters apparently remote from his main fields of interest. And as a result he formulated for himself and then followed that fundamental rule of research: *The first thing to do is to find out everything everybody else has done, and then begin there where they left off.*

It is only by reading the log-books of previous explorers in a field, in the form of articles describing their work, or by talking with them directly, that an investigator can find out what discoveries they made and where they left off. As pointed out by Crane and Patterson in *A Guide to the Literature of Chemistry*, the failure of an investigator to get all the information the records contain is likely to result (1) in the use of

inferior apparatus and methods of experimentation, (2) in poorly planned investigations, or, worse still, (3) in useless duplication of effort.

It is, however, a question whether an investigator should always begin the study of a new problem by making an immediate and exhaustive search of the literature. It appears to be best sometimes to do enough first-hand investigation or experimentation right at the outset to get a reasonable familiarity with the problem. If this is not done, it may easily be that the relative importance of the various points given in the literature can not be appreciated, with the result that during the search some of the significant items of information may be passed over altogether. Professor James R. Withrow used often to tell his classes in industrial chemistry at the Ohio State University that the very first thing to do in trying to solve any problem is to get a clearly defined conception of what the problem is. Not to do so would be just as though a doctor began treating a patient without first having found out what was wrong with him. But that is a thing which, sad to say, is sometimes done both in medicine and in research.

Nevertheless, in the undertaking of any new problem the making of a comprehensive survey of the literature is important, and it should be made early. Before undertaking any specialized investigations it is quite important to get a general picture of the surrounding field. If this is not done, the investigator is liable to the same kind of limitations as the six blind men of Hin-

dustan had in their investigation of the elephant. The one who happened to come at the side of the beast decided from his observations there that the elephant is "very like a wall." The second who chanced to feel the tusk decided that the elephant was "very like a spear." The third who came in contact with the squirming trunk concluded that the elephant was "very like a snake," and so on with the other three. But, though each of these imperfectly informed investigators was partly in the right, "all were in the wrong." So also is any research worker liable to be if the field of his investigation is too restricted. Hence it is that, besides the help it often gives in the prosecution of a specific investigation, paper exploration is important as well in giving the investigator a comprehensive view of the field within which his problem lies.

There is still another excellent reason for getting familiar with the prior art by reading scientific literature. It may save an investigator from doing an injustice to others and from the embarrassment that naturally follows the proud placing of the flag of discovery on a spot that has already been discovered by another. When Pasteur found the bacillus that manufactures butyric or rancid-butter acid, and discovered that it could live and do its work without any air, he announced to the world that "this is the first example of little animals living without air." Unfortunately for Pasteur's dignity, it was not, as Paul de Kruif pointed out, the first example, but the third. Pasteur had failed

to give credit to Leeuwenhoek, who had seen the same thing two hundred years before, and to Spallanzani who one hundred years before Pasteur had found that microscopic animals could live without breathing. Pasteur also rediscovered the fact that microbes make meat spoil, and failed to give proper credit to Schwann who was the first to make that observation. Knowledge of the prior work of these men, which could have been had by a little paper exploration, would have saved at once Pasteur's time and his dignity.

Although an investigator should get all the hints he can from reading about what others have done or from talking with them, he ought not to allow anything he reads or hears to discourage him nor to stifle his originality of thought or his initiative of effort. The mere fact that an experiment has been tried by some one with negative results is not necessarily a sign that that particular thing can not be done, if only conditions be varied a little. Before the time when Moses Gomberg was a student of Victor Meyer's at Heidelberg, many men had tried to make the hydrocarbon tetraphenylmethane. All the attempts had been so unsuccessful that Victor Meyer himself had become convinced that making that particular compound was impossible. But to young Gomberg the synthesis of tetraphenylmethane was not an impossibility, for he succeeded in making it.

Michael Faraday did not accept without reservation the results of any other investigator, even when they

turned out to be positive rather than negative in character. Faraday made it a rule to repeat for himself every experiment he wrote about or lectured about. It was this unusual habit of his that led him on to the monumental discovery of the hitherto hidden secret of electromagnetic induction.

There is much hearsay and speculation in the literature of science, just as there is in anything that people are connected with. In reading scientific papers it is therefore important to distinguish between original data or evidence and the speculations of the author about what the data may mean. Dr. Donald B. Armstrong, writing in the *American Journal of Public Health*, March, 1932, pointed out that at any time there are various grades of hygienic truth. And what he says applies equally well in every other field. First, there are *Grade A* facts. These are those which are fully demonstrated by ample and conclusive scientific evidence. Examples of such Grade A truths, as given by Dr. Armstrong, are the germ theory of disease, the value of vaccines and serums in smallpox, typhoid fever and diphtheria, the importance of minerals and vitamins in the diet, and the physiological value of sunlight. Second, there are *Grade B* facts or "near facts." These represent the consensus of expert opinion as to probabilities but they lack the full demonstration that comes from controlled experiment. Naturally it is quite important in searching the literature to make definite distinctions between opinion—even though it

be expert opinion—and facts which are supported by adequate scientific evidence. Finally, in Dr. Armstrong's classification of the grades of truth, there are the fallacies—the popularly held opinions that are, and that may have been shown to be, untrue. And how many of these latter there are, even sometimes among scientific men!

In making paper explorations, it is well to remember a saying of James Harvey Robinson's to the effect that, with most people, ideas like kisses go by favor. And so, although searching of the literature is of great importance, it must neither be allowed to take the place of experiment nor to discourage originality of thought or first-hand experimentation.

It was too much implicit reliance on the ideas of the past, notably those advanced by Aristotle, that prevented progress during the several centuries prior to the experiments of Galileo. People preferred speculating and debating on some subject that they really knew little or nothing about, such as how many angels could stand on the point of a pin, to quizzing nature by actual experiment. Besides preventing progress, this substitution of reading and debate for experimentation was a great loss in another way to those who did it, namely, in that reading about the experiments of another is less interesting than first-hand experimentation. The great Agassiz told how he learned that the study of things themselves was far more attractive than what is printed in books about them. "I usually contented

myself," he said, "with turning over the leaves of the volumes of natural history, looking at the illustrations, and recording the titles of the works, that I might readily consult them for identification of such objects as I should have an opportunity of examining in nature."

The making of a search of the literature, if it is a thorough search, is a very difficult matter. The difficulties arise chiefly because of the abundance of recorded information, of the large number of periodicals in every field, and of the limitations of language. The mere index to the literature of chemistry, for instance, covering the ten years 1917-1926 (Second Decennial Index of *Chemical Abstracts*), occupies 6,600 pages of fine print. In order to read the articles of which the abstracts are indexed there, one would need to have the volumes of over twelve hundred periodicals, and he would have to be able to read about twenty languages. Fortunately, however, the greater portion of the articles—perhaps 85 per cent of them—can be read if one has a command of but three languages, English, German, and French.

Besides the difficulties in reading scientific literature that arise because of differences of language, there is another which resides in the inadequacy of words in any language for the un-ambiguous expression of ideas. Even though the writer of an article takes the greatest of pains in phrasing it, which many of them distinctly do not, the text is nevertheless likely to be ambiguous

in two important respects. It is likely to be ambiguous, first, because every detail can not be covered, and second, because of what Willis R. Whitney has called "the fearful failure of words to cover ideas." It is in this latter connection that Whitehead has said, "No verbal statement is the adequate expression of a proposition." In connection with incompleteness of detail in technical papers, the story has been told that an unconscious, and therefore unreported, trick of the one-handed Dr. Noguchi—which trick consisted in carrying cultures from incubator to microscope in his vest pocket—prevented for a long time the confirmation by others of his experiments in cultivating the *Spirochaeta pallida*. So, altogether, effective paper exploration is not without its difficulties.

What the best method of making a paper exploration is, depends upon what kind of information is wanted. There are no set rules. The object of the search may be no more than to find the boiling point of some compound or the strength of some material. On the other hand, it may be an extensive investigation of the relationship of boiling point to composition and structure, or of the subject of strength of materials in general. Although effective paper exploration is an art for which it is not possible to lay down hard and fast rules, an excellent set of hints on how to go about a search of the literature on any subject is given by Crane and Patterson in *A Guide to the Literature of Chemistry*.

CHAPTER VIII

OBSERVATION

MANY years ago, when I was living in College rooms," said Sir J. J. Thomson in one of his radio talks, "I happened to tell my bed-maker that at Oxford they had men scouts instead of bed-makers. She said she was sure if that were so the staircases would be very dirty. In true scientific spirit she determined to test her theory, and went to Oxford when next there was an excursion. The next morning when I saw her she was in great glee. She said: 'I was right, Sir, about the staircase. I went up every staircase I could find, and they were much dirtier than ours in Trinity.' I asked her what she thought about Oxford, which she had never seen before, and I found that the buildings, the courts, the walks and the river had left absolutely no impression upon her. She had just regarded them as obstacles in the way of getting at staircases on which she had concentrated. She had proved her theory, but she had missed Oxford."

This story illustrates the lack of a thing which is of great importance in research, but which research workers sometimes lack also, namely, comprehensive observation. In experimentation it is naturally important to see the result that the experiment is directed to-

wards; but that is not enough. Nature so often hands a fellow a left-handed gift, and only a left-handed one, that it is nothing but the part of wisdom to keep an eye on her left hand as well as her right.

It was left-handed gifts of the kind meant here which led to Faraday's great discoveries in electromagnetics. Like other investigators before him, Faraday tried to make a wire carrying an electric current rotate on its own axis. Like his predecessors, he was altogether unsuccessful. But, unlike them, he discovered that nature was willing to make a wire carrying a current rotate around a magnet, or a magnet around the wire.

Faraday knew also that many good men had tried to set up or induce a current in one wire by passing a current through another near it, and had failed. Nevertheless he must try it for himself, and he persistently did so on more than one occasion. He wound two insulated wires around the same wooden cylinder. One of the coils he connected with a battery of 10 cells, and the other with a galvanometer. But no current was induced in the twin coil. The needle of the galvanometer rested right at zero. He gradually increased the number of cells from 10 to 120. In spite of the large current then flowing through the one coil, however, none that would deflect the needle of the sensitive galvanometer was set up in the other.

But although the steady current flowing in the primary coil failed to produce any in the secondary, Faraday's observing eye noticed a slight kick of the gal-

vanometer needle just as he closed and again as he opened the battery circuit. This unexpected observation caused Faraday to make further experiments which showed that a *change* in the current flowing in the primary coil, not a steady current, would induce a current in the secondary coil. These experiments then led on to his crowning discovery of how to induce one electric current by another.

Charles Darwin attributed much of his own scientific success to his ability "in noticing things which easily escape attention, and in observing them carefully." And it is often the exceptional observation that is most worth while. Modern meteorology is said to have originated from an observation made by Benjamin Franklin as a result of his personal correspondence with friends in Boston. He noticed that, quite contrary to the accepted opinion of the time, storms traveled in the opposite direction to the wind, or that northeasters sometimes reached Philadelphia before they did Boston.

The great value of tungsten as a constituent of steel was discovered about sixty-five years ago when Robert Mushet, a Sheffield metallurgist, noticed that a piece of steel he happened to be working with did not need quenching to harden it. He had the piece analyzed and found that it contained tungsten, a rare metal which up to then had had no place in metallurgy. Argon was discovered in the air as a sequence of Lord Rayleigh's having noticed that a given volume of nitrogen derived

from the air always weighed a small fraction of a grain more than the same quantity of nitrogen obtained as a chemical by-product from other sources. The suggestion of the use of wood for making paper came not from a chemist but from a naturalist of the early eighteenth century, who made it as a result of watching wasps build their nests.

Henry E. Armstrong, writing in *Nature* about Ludwig Mond, told the following story of how the famous Mond process of withdrawing nickel from its ores by means of carbon monoxide gas came to be. Ludwig Mond was trying to develop the Grove hydrogen-oxygen, sulphuric acid, platinum, gas cell. For this purpose he needed hydrogen in considerable quantity. Water gas was chosen as the source of this hydrogen, but it first had to be purified by the removal of carbon monoxide, which is a poison to platinum. The purification was accomplished by the known method of passing the mixture of gases over heated nickel. At intervals during the experiments Mond's assistant, Dr. Quincke, in order to prevent carbon monoxide from contaminating the air, put the escape tube from the apparatus into one of the air holes at the base of a Bunsen burner. He noticed that whenever he did that the burner gave a smoky flame. At once Mond followed up this chance observation to the discovery of the presence there of the gaseous compound of nickel, nickel carbonyl. He then found that simply by heating nickel carbonyl to a temperature somewhat above that at

which it is formed it is easily resolved into metallic nickel, with the liberation of free carbon monoxide again. These discoveries led him quickly to the development of the continuous, and ever since largely used, Mond process of withdrawing nickel from its ores through the simple agency of carbon monoxide gas.

A steel company had been experiencing trouble from variations in cleanliness among different batches of steel. In the effort to run down the cause of the mysterious difficulty, an observing investigator noticed that the batches turned out by tall men were uniformly cleaner than those turned out by the short men. But what had the height of a steel worker to do with the quality of the metal he produced? Upon carefully following up the apparently unrelated observation, it was found to be simply this. In the stirring-in process which followed the introduction of the deoxidant into the melt, the short men could not reach high enough to work the deoxidizing agent clear down to the bottom of the container. So the tall men who could do that were the only ones who produced uniformly clean batches. All that proved necessary to correct the trouble, following the careful sleuthing which showed the cause of it, was to provide each of the shorter workers with a little elevation to stand on while he stirred the deoxidizing agent into the molten steel.

It has been said that the chief disqualification of the alchemist was too much imagination and too little

observation. In distinct antithesis to this is one of Charles F. Kettering's rules of research. The way to find out about a thing, to solve a problem, or to improve upon something, says Mr. Kettering, is to begin experimenting with it. By so doing the experimenter puts himself into a position where the lightning of an illuminating observation may strike him. During the period of his experimentation, in other words, enough things usually happen which, if noticed and carefully followed up, lead the experimenter on to the object of his search, or even to a better one still. But, of course, when he sets out on such a route of experimentation he should not leave his imagination at home. He will need all the imagination he can muster as well, both in deciding on what experiments to carry out, and in getting illuminating suggestions from the trivial appearing events that result from them.

In a letter written to his son in 1871, Charles Darwin said: "I have been speculating...what makes a man a discoverer of undiscovered things, and a most perplexing problem it is. Many men who are very clever—much cleverer than the discoverers—never originate anything. As far as I can conjecture, the art consists in habitually searching for the causes and meaning of everything which occurs. This implies sharp observation and requires as much knowledge as possible of the subject investigated."

It was because he was what Darwin called "an incomparable observer" that Jean Henri Fabre was such

a great naturalist and so successful a one. And therein lies the obvious moral to all that is said above, namely, that the research worker must diligently put himself in the way of making observations, and that while he is there he must let no event pass unnoticed; for, in science as elsewhere, seeing is not necessarily believing—nor is it even noticing.

CHAPTER IX

ACCIDENT

IT was an apparently accidental happening, relates Ellice McDonald, which led to the use of radium in the treatment of cancer. The French physicist Becquerel once carried a little radium, then a strange new substance, in his pocket for a time. He soon found that the effects of the radium destroyed the tissue of his skin near it, making a spot like an ulcerated sore. And it was that apparently chance observation of the destructive effect of radium on human tissue which led to the idea that radiation from radium might be of use in the treatment of cancer.

The element of accident—or, as some call it, luck—has always had a large place in discovery and even in the results of organized research. It was the accident of bringing a mixture of caoutchouc and sulphur “in contact with a hot stove” that pointed Charles Goodyear to the way to vulcanize rubber. Friedrich Wöhler in his laboratory was evaporating down a water solution of ammonium cyanate that he had prepared, when altogether unexpectedly he obtained urea instead, and thereby became the accidental founder of the great science of organic chemistry. W. H. Perkin was trying to make quinine by oxidizing aniline oil when he got

what organic chemists often get—a black, tarry mass. He then happened to use alcohol to wash out that vessel, and was surprised to get a beautiful purple solution. What he had was later named “mauve,” and became the first one of the aniline dyes.

Roentgen discovered X-rays because an exposed negative carefully wrapped in black paper, which happened to be lying close by while he was experimenting with a Geissler tube, became light-struck so that the picture it contained in embryo was spoiled. The accidental observation, made in a Chicago greenhouse, that one part of illuminating gas in eighty thousand parts of air caused carnations to “go to sleep” led to the discovery of ethylene as an anesthetic. Blotting paper was discovered by chance when a workman in a paper mill forgot to put in a certain sizing. When some one later tried to write on that paper the ink spread all through it. Then it occurred to him that paper of that kind would be better than sand for drying up ink.

The fundamental principle of the telephone was discovered when the clockspring reeds of an experimental telegraph transmitter accidentally stopped vibrating as Alexander Graham Bell and Thomas A. Watson were trying to develop Bell’s idea of a musical telegraph. The reed was held down by its electromagnet, and Watson in trying to pluck it free made it move up and down over the electromagnet, thereby varying the current that was flowing continuously in the line, and Bell at the other end of the wire heard the faint cry that

resulted from the undulating current thus sent over the wire. After having labored for two years in the effort to make an incandescent lamp, Thomas A. Edison was toying one night with a piece of lampblack mixed with tar (prepared for use in his telephone transmitter) when, as he rolled it between his thumb and forefinger, the inspiration came to him that a thin spiral made of that material might be a good filament for the incandescent lamp. He tried it, and sure enough it worked. That particular material did not prove to be good enough to make filaments out of, but it started Edison off on the road to the successful carbon-filament lamp.

In view of how largely the element of accident thus looms in research, it is indeed a question whether most discoveries are not made more or less by accident. It is true, at any rate, that in the story of research "The chapter of accidents is the longest chapter in the book." But this is in no sense a discredit to the men who have made great discoveries or outstanding scientific advances. It is nearly always after much intelligent and patient endeavor that the accident, if such it may rightfully be called, occurs. "It is on the knees of the gods whether there will be a loose ball in the football field in any particular game," says Wilder D. Bancroft, "but that accident will not mean a touchdown unless there has been a great deal of preliminary training."

"A man may not find what he is looking for," wrote Dr. Slosson, "but he never finds anything unless he is looking for something." It is usually the trained and

alert observer who is able to recognize the importance of apparently accidental happenings, and to seize upon the useful suggestions that they give. It is somewhat like getting the solution of a difficult mathematical problem. In mathematics the solution sometimes comes to mind like an unexpected inspiration. But, although unexpected, it comes only after much previous pondering and pencil sharpening.

If Becquerel had not been experimenting with radium, he would not likely have made the observation which led to the use of radium in the treatment of cancer. A great many physicists besides W. C. Roentgen were trying to photograph electric discharges through vacuum tubes back in the 1890's; and others besides Roentgen, among them no less a one than Sir William Crookes, had experienced trouble with fogged plates. Crookes cured the trouble by keeping his supply of photographic plates in an adjoining room. But Roentgen put aside the immediate job he was working on and took time to find the source of the strange invisible rays that were fogging his plates. And so it was that he became the discoverer of X-rays. If Alexander Graham Bell had not been looking for a means of carrying the human voice over a wire as well as of improving the telegraph, it is not likely that he would even have recognized the importance of the means of varying the current in an electric circuit that had so accidentally been demonstrated to him, much less have seized upon the great discovery and built a successful

telephone with it. Because he frequently needs to take advantage of fortunate accidents' is thus one of the reasons why the successful research worker has to be a good observer.

The agar-agar industry of Japan is said to have been founded upon the accidental discovery of a humble Japanese mountaineer, who was observing enough and scientific enough to take advantage of his chance discovery. He was sitting close to his stove one cold evening, so the story goes, when there came a knock on the door of his hut. Opening, he found standing there his Emperor, the Son of Heaven, with his retinue. They were lost in the mountains, shivering from cold, and hungry. The humble man was so honored by the visit from his Emperor that he set before him an unusually large dish of his best seaweed jelly. When the meal was over the man, in deference to deity, threw out what the Son of Heaven had not eaten. Next morning after the Emperor had departed, the mountaineer went to get his discarded delicacy again. But it had been frozen during the night. As the frozen jelly thawed out in the morning sun, it all disintegrated and water separated from it in a little pool. What was left was a light fluffy mass looking not at all like jelly, but more like paper flowers. In the effort to restore his jelly, this experimentally-minded mountaineer heated the residue in water, saw it return to jelly again. From this incident is said to have grown the agar-agar industry of Japan.

What has been said above has all been concerned with the part that seeming accident sometimes plays in the *making* of discoveries. But to the accidental in research there is another side as well, namely, the part that it plays in the *missing* of discoveries. There are unlucky accidents as well as lucky ones. It is no doubt often that some apparently insignificant or entirely overlooked factor causes a negative result to be secured instead of the positive one that slightly changed conditions might have yielded. If you make an experiment to test out the validity of an idea, says Charles F. Kettering, and the experiment fails, then you ought to be careful to find out just why it failed, because the failure may not have had anything at all to do with the reasonableness of the principle. It may have been simply that you did not have the right experimental conditions or procedure.

When Thomas Midgley, Jr., first tried to make lead tetraethyl, which later became the essential ingredient of Ethyl gasoline, he failed; and his failure was on account of an accidental happening. He tried to prepare lead tetraethyl by the method of Löwig, which consisted in the interaction of ethyl iodide and a sodium-lead alloy, followed by extraction with ether. But Midgley got no lead tetraethyl at all. Later on, after he had made lead tetraethyl by another route, he discovered that the reason for his failure in the first method was that he had used dry ether for the extraction, whereas Löwig had evidently used ordinary wet ether,

as a little water for hydrolysis was necessary to the formation of lead tetraethyl.

Paul de Kruif tells in *Microbe Hunters* that when Louvrier was trying to demonstrate to Pasteur that he had a means of curing anthrax, Pasteur said, "Now, Doctor, choose two of these sick cows—we'll call them A and B. Give them your new cure, and we'll leave cows C and D without any treatment at all." The result was that one of the cows treated got well, and the other died; also, one of the untreated cows got well, and the other died. "This experiment might have tricked us, Doctor," said Pasteur. "If you had given your treatment to cows A and D instead of A and B—we all would have thought you had really found a sovereign remedy for anthrax." The moral to this is plain enough, but may as well be stated anyway: if the research worker is to keep his experiments from tricking him, he must take care to surround them with every precaution and every safeguard.

CHAPTER X

RE-SEARCH

NOW search may not be bad, but research—do it all over again—that is surely something to make the gods smile.” So said Hamilton Holt recently. If Dr. Holt was right, the gods must have a perpetual smile on their faces, for it is only by the most careful and repeated search—by search and *re*-search—that things worth while are found out and established for sure.

Every successful investigator does his experiments “all over again,” and again and again. Observations must be checked and rechecked, and checked again. Even then it is hard enough to be sure; for, as it is put in the Gilbert and Sullivan opera *Pinafore*, “things are seldom what they seem.” Even the plain truth can be misleading at times, just as it was in the story of the sea captain who himself was a teetotaler, but who had as a mate a too hard-drinking man. The captain tried to cure the mate of drinking by entering in the log from time to time for the information of the ship’s owners, “Mate drunk all day.” As a retaliation for this, the mate surreptitiously wrote in the log on such intermittent occasions as he had an opportunity, “Captain sober to-day.”

Speaking of the first of the “Microbe Hunters,” Paul

de Kruif said, "There never was a less sure man than Leeuwenhoek. He looked at this bee's sting or that louse's leg again and again and again." The great anatomist, John Hunter, who "raised surgery from the art of the barber-surgeon to the dignity of a science," said in a letter of directions to Edward Jenner, the discoverer of vaccination, about certain experiments he wished him to make, "Be as particular as you possibly can." Surely nothing less than *re*-search would comply with that instruction. The research worker should have the philosophy of Descartes: doubt everything; do so, however, not in the futile manner of the chronic disbeliever, but merely as a means of finding those things which are beyond doubt.

"We are not infallible," said Michael Faraday, "and so we ought to be cautious." How cautious one great scientific explorer, Sir Humphry Davy, was may be seen from the following quotation from the paper in which he described his discovery that chlorine is an element, and not, as had been supposed, a compound of oxygen with hydrochloric acid: "In the views that I have ventured to develop neither oxygen, chlorine nor fluorine are asserted to be elements; it is only asserted that, as yet, they have not been decomposed."

This matter of the definition of the limits of justifiable conclusion is an important element in research. Thus it was thought for many years that a mixture of carbon monoxide and oxygen when thoroughly dry or free from water could not be ignited or burned. This

surprising observation of the original investigator was checked by later experimenters, who also were unable to ignite dry carbon monoxide. But still later on came an experimenter who, using a different or more adequate ignition system, showed that a mixture of carbon monoxide and hydrogen *can* be burned, no matter how well dried it is. The observation of the earlier investigators should have been not that drying such a mixture makes it *incapable* of being ignited; but only much *more difficult* to ignite. That much was certain, and it was all that their experiments had really shown for sure.

As a means of illustrating his belief that the truths, or the half-truths, of science are often dangerously misleading, G. K. Chesterton recently told in the *North American Review* a parable which was to the following effect. Professor Higgins establishes the great discovery that the nose is the chief nest of microbes. "Professor Biggins ratifies and illuminates the discovery of Professor Higgins, by pointing out that the nose is so unquestionably the most attractive seat for germs that such and such an enormous portion of all the germs in the body are to be found there." The result is that soon, in order to eliminate this nest of germs, certain bold spirits begin cutting off noses, as people cut out appendices. So general becomes the practice that it is suggested that the State lend itself to a sweeping scheme of amputating noses, like the scheme of vaccination.

But at this point along comes Professor Miggins, working on the same lines as previous investigators, and makes a striking discovery. He finds that, in the case of those still fortunate enough to have noses, "there are practically no germs in the rest of the body; and that this is due to the nose holding them up or shutting them out, so that it acts as a prison for undesirable aliens. Professor Jiggins clinches this by triumphantly showing that since noses have been abolished, germs have multiplied in thousands all over the rest of the human organism. The thing described as the Nasal Barrier by Professor Jiggins, in his address before the Royal Society, has been broken down; and life is flooded with millions more murderous microbes than before."

The unfortunate situation pictured in the Chesterton parable is, as the author of it said, an extravagant one. But it does point out the need for caution in science. "There is nothing more dangerous in philosophical investigations," said Count Rumford, "than to take anything for granted, however unquestionable it may appear, till it has been proved by direct and decisive experiment."

The reason for this lies in what Paul de Kruif calls "the infinite complicatedness of everything." Things are all so intimately linked together that, without direct and comprehensive experiment, it is impossible to tell where the effects of even a simple change will go nor how far they will extend. This is illustrated by J.

Arthur Thomson's account, based upon Darwin's famous "cats and clover" story, of how it is that kindly old ladies have a part in insuring the supply of milk and roast beef. The more kindly old ladies there are the more cats there will be, and the more cats the fewer field mice. The fewer field mice the more bumblebees, for field mice destroy bumblebees by robbing their nests of the delicate white grubs that represent the next generation of bees. The fewer the bumblebees the poorer will be next year's clover crop, because no other bees visit the clover blossoms and carry the fertilizing pollen from flower to flower. The more clover the richer the pasture for the cattle, and so the better the supply of milk and roast beef. That this house-that-Jack-built sort of story is representative of what happens in nature can be verified by any one who has ever worked at research and has been impressed by the long chain of unexpected events brought on by some simple change that he has made in something he was working on.

All of this is why the automobile industry has proving grounds; it is why in developing a new manufacturing process the chemical industry goes step by step from the test tube to the flask, from the flask to the semi-works plant, and finally from the semi-works plant to one unit of the full sized plant; and it is why new developments in medicine have to be put into use with the utmost caution.

People are always looking for the invariant or the

infallible to lean upon. This is true in religion, in education, in business, and in politics, as well as in science. But the invariant is either difficult or impossible to realize, for it implies perfection. It would not be wise to wait for perfection before putting a new discovery to use, for that would seriously retard progress. But in research it is of the highest importance to nail down every observation with all the certainty that is possible. Otherwise the final result may be like Mr. Dooley's book. Mr. Dooley said that in his youth he wrote a book about women. Some years later, desiring to reprint it, he first gave the text a critical reading, and as a result he inserted in the front under the heading *Errata*: "Wherever in this volume appears the word *is*, substitute *is not*, and wherever the words *is not* appear, substitute *may be*, *perhaps* or *God knows*."

CHAPTER XI

FINANCING

GELD, *Geduld, Geschick, und Glück*. These—gold, patience, cleverness, and luck—said Paul Ehrlich, were the four big “G’s” needed in finding that “magic bullet” of his, which he called salvarsan, or 606. Gold, or financial backing, must be provided for any exploration, whether of the geographic or the up-to-date kind. And how to get that backing has always been one of the problems in exploration.

Securing financial support for his expedition was one of the first problems that Columbus had to solve. After much searching, he finally found a patron in the idealistic Isabella, queen of Castile, who even offered to pawn her jewels to finance the undertaking, in case the Treasury funds should prove to be inadequate. It is worth noting, however, that this promise of the Queen’s came only after six years of negotiation and sales effort directed at her by the zealous and persistent Columbus. He had tried to sell his idea of going west to several others as well, including the king of Portugal and even King Henry VII of England. In spite of the great difficulty he had in securing support for the undertaking, figures have been advanced to show that the total cost of Columbus’s voyage was only about

\$5,500. But, of course, the amount of the support needed is not necessarily a measure of the difficulty of securing it.

In the field of research the free-lance experimenter of the past either had to find a patron who would support, or at least help support, his endeavors, or else he had to be his own patron. Thus, as patron of his experiments, Watt had his Boulton, Morse his Vail, Bell his Hubbard, and Fulton his Livingston. But Palissy and Priestley and Franklin and Goodyear and Davenport and the Wright Brothers were their own patrons. As a consequence, some of them passed through periods of actual destitution. In his experiments on pottery, Palissy was finally reduced to using the flooring of his house as fuel for his kilns. Thomas Davenport spent all the resources of his smithy in the effort to produce "rotary motion by repeated changes of magnetic poles," or in common parlance to make an electric motor, at last having to tear his wife's silk wedding dress into strips for insulating the coils of his experimental motors. The airplane flew out of a humble bicycle shop, for it was by repairing bicycles that the Wright Brothers supported their long efforts to fly.

The great German chemist, Justus von Liebig, wrote in 1824:

If you consider that in a scientific paper practically every word means an experiment and every experiment requires the use of chemicals and apparatus, you well ask where I obtained the funds required. [His salary as professor in the

University at Giessen was then 120 dollars a year.] I will tell you—the money was borrowed....I was like an alchemist who, in addition to sacrificing his health, puts his own wealth and all that should belong to his wife and children into the crucible.

The organization of research with a definite operating budget on the same basis as other departments of a business, such as the accounting, the purchasing, or the sales, has in large measure done away with the starving experimenter. But, as has already been said, such organization has only come about to any appreciable extent within the generation just past. By 1932, however, aside from educational institutions, foundations, and the like, there were about sixteen hundred organized research laboratories in the country. One of these, the Bell Telephone Laboratories, has a personnel of three or four thousand persons and an annual operating budget of several million dollars. E. W. Rice, Jr., of the General Electric Company, whose company has more than one research laboratory, said recently, "We have spent well over \$100,000,000 in the past five years on our development work and we intend to continue at whatever rate of expenditure seems to be necessary. For we have found no other money so well invested."

That last sentence of Mr. Rice's suggests one of the reasons why the organization of research has grown by leaps and bounds during the past twenty years. If investment in general is a gamble, then an investment in a properly organized and managed research laboratory

is one of the best gambles known. The chances of winning are good, the winnings may be large with relation to the amount invested, and quite often they are not got at the expense of some one else, but instead represent the production of altogether new values. "I know perfectly well from my own experience and my own direct observations," said Frank B. Jewett, "that research properly organized can be made profitable in many fields of industry. I have reason to believe that it can be made profitable in every line of endeavor."

It should not fail to be noted, however, that, in order to be useful, research must have proper organization, adequate financing, and intelligent direction. The mere haphazard doing of research is no insurance of success at it. Essential to success in research also is the element of time. "Too frequently," says W. A. Gibbons of the United States Rubber Company, "organizations have been willing to spend money on research, but have not been willing to spend time." L. V. Redman estimates that, from the time a research endeavor is begun until practical success is finally attained, there may be a period of seven or eight years. That is why the late John E. Teeple spoke of the need in research for "patient money."

A. F. Woods, of the Department of Agriculture, in a radio address said that the states and the United States together have been spending about thirty million dollars a year in the development of the nation's agricultural industries, which have a value of sixty

billion dollars and a gross income of ten billion dollars a year. Thus the expenditure on research of one-fourth of one per cent of its annual income, or one-twenty-fifth of one per cent of its invested capital, over a long period of years has made American agriculture, with all its shortcomings, the best in the world. The same thing may be said about other American industries, although there the expenditure has not been made by the government for the most part. It has been estimated that the total annual expenditure on research in the United States is now around \$200,000,000, which represents just about one-fourth of one per cent of the total income of the country in normal times.

With regard to estimates of the amount of money spent on research, however, it should be said that the amount of effort put into research is very difficult to get with accuracy, and is quite probably underestimated. The cost of research is usually given as the amount of the expenditure on definitely organized research. But in any progressive enterprise there is much experimentation and even actual concentrated research being done in scattered units of the business by men regularly employed at something else. And such experimentation is often of the highest value. Thus, one of the greatest contributions of all to the motor car has been the work done largely by men in manufacturing units in so improving methods of fabrication as at once to make possible better cars and large reductions in the cost of cars to the customer.

The organization of research on a high plane from the standpoints of men and equipment—particularly from the standpoint of men—and the provision of proper financing to keep it there are essential, if best results are to be secured. Thus, for instance, it has been pointed out that, of the ten grants of the Nobel scientific prize to men in the United States thus far, one-half have gone to men associated with one or another of the excellently organized and adequately financed research institutions supported by the John D. Rockefellers. These have been as follows: Albert A. Michelson (Physics, 1907), University of Chicago; Alexis Carrel (Medicine, 1912), Rockefeller Institute for Medical Research; Robert A. Millikan (Physics, 1923), who went to California Institute of Technology in 1921 after having worked twenty-five years at the University of Chicago; Arthur H. Compton (Physics, 1927), University of Chicago; Karl Landsteiner (Medicine, 1930), Rockefeller Institute for Medical Research. The winners of the five Nobel scientific prizes in science which have thus far been granted to men in the United States not attached to a Rockefeller-supported institution were received by Theodore W. Richards (Chemistry, 1914), Harvard University; Irving Langmuir (Chemistry, 1932), the General Electric Research Laboratory; Thomas Hunt Morgan (Medicine, 1933), California Institute of Technology; George Hoyt Whipple, George Richards Minot, and William Parry Murphy, jointly (Medicine, 1934), the first of the Uni-

versity of Rochester School of Medicine and Dentistry and the last two of Harvard Medical School; and Harold C. Urey (Chemistry, 1934), Columbia University.

That in the industrial field, as well, the most prolific research laboratories are those which are properly manned and adequately financed is indicated by the history of industrial research over the past twenty-five or thirty years. The industries which have made the greatest strides forward during that period are precisely those which have gone in for research on the soundest and the most extensive basis. Those industries are chiefly the chemical, the electrical, the telephone and such allied communication industries as radio and sound recording, the photographic, and the automotive. Every one of these outstanding industries was not only founded upon research but also its phenomenal progress resulted largely from constant improvement through continued research. In a recent report to the National Research Council, an American chemical manufacturer said, "Research is the one tool by which within the short space of fifteen years American chemists, engineers and physicists have established in America an organic chemical industry, the magnitude of which is so great and the quality of whose product is so good that it is the marvel of our European competitors."

As to how much money any given industrial enterprise ought to spend on research, it is not possible to say with definiteness. That depends upon too many conditions, such as the character of the business, its age,

the nature of its products, and the like, to make it possible to set up any definite standard. Different companies now spend on research anywhere from almost nothing at all up to 5 per cent or more of their invested capital. The average expenditure on research of all those companies which maintain research laboratories, as indicated by a recent survey conducted by the National Research Council, is 1.3 per cent of the capital invested. However, there are reasons why that figure, although the best that could be got, should be taken with a grain of salt, and perhaps with more than one grain. One of these reasons is that not all those companies which carry on research reported their expenditures. Another reason arises from the situation, as suggested above, that much of the valuable experimentation which goes on within an organization is done by alert men in various departments of the business who are not classified as research workers at all.

The best guide to the amount that ought to be spent on research in any business is to be obtained from a careful study of how much research is needed for the good of that particular business. Charles F. Kettering has suggested that a manufacturer ought to spend on research as much as he spends on advertising. If every manufacturer should do that, there would be an increase in research activity amounting at least to five-fold and perhaps more, although information on the amount of money spent on advertising is about as indefinite as is that relating to expenditure on research.

Setting up a budget for research—and the best way for a research laboratory to be financed is on the basis of a definite operating budget—is comparatively a simple thing, as the cost of research consists very largely of the salaries of the necessary workers. And, since the output of the laboratory depends in turn upon the quality of the workers within it, the importance of making the salaries adequate to attract and hold good men is apparent.

“One cruiser for cancer would insure the banishment of this plague.” This startling statement regarding the probable cost of finding a cure for cancer was made in 1931 at a meeting of the American Chemical Society in Washington by Ellice McDonald, director of cancer research at the University of Pennsylvania. How very cheap that would be! But still we go on building warships, while each year thousands of people suffer and die with the dread disease of cancer. “I beg you,” the great Pasteur plead with the French people, “take some interest in those sacred dwellings meaningly called laboratories. Ask that they be multiplied and completed. They are the temples of the future, of riches and comfort.”

CHAPTER XII

SELLING

THE selling of a new idea is even more important than the getting of the idea, suggests Charles F. Kettering. The reason is that an idea does the human family no good until it has been sold to some one. And to sell a new idea, further suggests Mr. Kettering, usually takes a great deal of effort, ingenuity, and time. When a new idea is first laid on the table, it is generally pushed off at once into the waste-basket. But do not get discouraged at that, Mr. Kettering encourages. You know then just where the idea is, and you can go and get it out again. Besides, that is only the first time it was pushed off. Lay it on the table once more. And after you have done that time and time again, perhaps for three or four years, some one will begin to take an interest in it. No one who is temperamental should be in research, warns Mr. Kettering, because experience has shown that new ideas are consistently shoved into the waste-basket.

The world must be educated up to any discovery, agrees Wilder D. Bancroft, in speaking of the importance of salesmanship as a quality of the research worker. The man of science "should not rank below the cuckoo and the cowbird which at least picks out

foster mothers for its young, while the scientific man casts his ideas out into the world to shift for themselves. Since the greatest discoveries are likely to be the ones for which the world is least ready, we see that the greatest scientific men should really be super-salesmen."

Some one has said that "people are down on what they are not up on." It seems perfectly natural that a fellow should not be much interested in something that he does not know about and that he can not see the advantages of. Experience has shown as well that people are sometimes antagonistic to things they do not understand. The latter feeling, Mr. Kettering says, is usually not based on thought at all, but is simply a natural instinctive animal reaction against things new and unfamiliar.

About thirty years ago Hugo Eckener was a writer on political economy, with special reference to current topics. One of the things he wrote about and agitated vehemently against was the use of public funds in the support of Count Zeppelin's experiments on lighter-than-air craft. He contended that such ships would never justify the money spent on them.

One evening Eckener had a visitor who introduced himself as Count Zeppelin. During the conversation which followed it developed that both men were chess players, and thereafter they played many a game of chess together. But more vigorous than the Count's attack on Eckener's king was that which he made on his opinions about the airship. Finally, Count Zeppelin

suggested that Eckener take a ride in one of his ships. Eckener had by that time become so much interested that he agreed, and as a result was impressed to such an extent that he executed a right-about-face, and thereafter used his potent pen in the aid of Count Zeppelin. Ultimately he became the successor of Count Zeppelin, or the great foster-father of the airship.

One of the early developments of Charles F. Kettering in his research for the National Cash Register Company was a spring-powered cash register, in which the energy for driving the registering mechanism was furnished by the effort of closing the drawer, stored up and transmitted later through a spring. But, as is often the case with new things, there was opposition to the spring-operated register, particularly among men in the National Cash Register Company itself. One day at a meeting one of the men made a speech outlining his objections to a spring-powered register, and endeavoring to show that the adoption of such a device would be a mistake for a number of reasons, one of which was that springs subjected to such continuous service could not possibly hold up. After the objector had finished his remarks and sat down, Mr. Kettering, who was present, answered by asking the objector if he had a watch.

"Yes, I do have one," replied the objector, who, as Mr. Kettering and most of those present knew, happened to be a crank on watches and prided himself on the accuracy of his timepiece.

"What makes it go?" asked Mr. Kettering. The analogy suggested by that question was so obvious that all the objecting one got was a laugh from everybody present. And the effect of the laugh was to dispose of the opposition to the building of a spring-powered register.

"The fates were kind to Bell" when it came to the selling of the idea of the telephone. True, as Thomas A. Watson said, the president of the Western Union Telegraph Company "refused somewhat, contemptuously, Mr. Hubbard's offer to sell him all the Bell patents for the exorbitant sum of \$100,000." But at the Centennial Exposition in Philadelphia in 1876, Hubbard got a small table in the Education Building for the exhibition of Bell's telephone. At first no one visited the exhibit at all. But Bell was there waiting patiently for a visit from the judges of the exhibit. They came one day just at dusk. "One or two approached the table, picked up the instrument, fingered it listlessly," wrote Floyd L. Darrow in *A Popular History of American Invention*.

As the judges were about to pass on there was enacted a scene worthy of the brush and genius of a master artist. Dom Pedro, the young emperor of Brazil, followed by a company of gaily attired attendants appeared, and, rushing up to Bell, greeted him with great fervor. Dom Pedro had visited Bell's school for deaf-mutes years before and had been pleased by his system of visible speech. He was intensely interested in the new invention. Walking to the other end of the line, Dom Pedro placed the receiver to his

ear. Bell spoke and the emperor dropped the instrument, exclaiming, "My God, it talks."

There in the twilight stood the judges, awed and silent witnesses of this picturesque but momentous event. One by one they came forward, . . . each in his turn eager to test this latest marvel of science and invention. There were Joseph Henry and Sir William Thomson (Lord Kelvin), the latter declaring it to be "the most wonderful thing he had seen in America." From that moment Bell's telephone became the most popular exhibit of the exposition, and overnight its inventor leaped to world fame.

These events, coupled with the many popular lectures on and demonstrations of the telephone which Bell later gave with the aid of Watson in the effort to solve his pressing financial problems, so familiarized people with the marvels of speaking over a wire that, as Watson said, "The public was ready for the telephone long before we were ready for the public."

The failure of research workers to sell their discoveries has often long delayed their becoming of use to people. But, incidentally, it should be said that sometimes such failure is not due so much to lack of sales ability as it is to the failure of the discoverer himself to appreciate the uses that his discovery might be put to. It was in 1800 that Humphry Davy discovered that nitrous oxide would put people into an insensible sleep; but forty-four years went by before it began to be used for that purpose by Dr. Horace Wells, a dental surgeon of Hartford, Connecticut. J. Willard Gibbs published in an obscure journal, the *Transactions of the Connecti-*

cut Academy, under the title "On the Equilibrium of Heterogenous Substances," his important discovery of the Phase Rule ten years before it produced any effect. Even then it did so very slowly, and only through the efforts of other men. It was in 1811 that Avogadro announced his discovery of the law that equal volumes of gases at the same temperature and pressure contain equal numbers of molecules. But, as Wilder D. Bancroft has pointed out, Avogadro's law, although taught now to every freshman student of chemistry, was neither understood nor appreciated by chemists until after the reform of Cannizzaro in 1858. The so-called Edison effect, that blue glow which sometimes surrounds hot metal filaments through which electricity is flowing and which underlies virtually the whole art of the vacuum tube as used in radio and in telephony, was discovered by Thomas A. Edison in 1883, and patented in 1884. But nothing came of it until twenty years afterwards, because neither Edison nor any one else at that time understood it or saw any of the great uses to which it could have been put.

One ability which it is of particular importance for research workers to have is the power of putting their reports, whether oral or written, into the language of those to whom they are addressed. No other language is any good. The founders of the early Christian Church understood this, for it is recorded that every man heard them speak in his own language. Should scientific men do any less? But, in spite of the apparent obviousness

of the answer to that question, it is a fact that many of them do less than that, often much less.

Glenn Frank said recently that "the future of America is in the hands of two men—the investigator and the interpreter.... We have an ample supply of investigators, but there is a shortage of readable and responsible interpreters, men who can effectively play mediator between specialist and layman." And who should be or is likely to be a more responsible interpreter of the results of research than the research worker himself, if only he can say what he has to say in terms which are easy for his listeners to understand—or, better still, which are difficult for them to misunderstand.

In addition to the ability to tell other people about the results he gets, it is worth while for the research worker to have some understanding of the psychology of salesmanship. And, when it comes to the selling of ideas as distinguished from the selling of more tangible things, there is one of these principles in particular that is worth mentioning here. There is no more certain method of getting a man to accept an idea than to give him some reason for thinking that the idea is his own, or at least partly his own. People are always partial to their own children. Speaking along this line in one of the reports of the American Management Association on research activities in business organizations, Z. Clark Dickinson says, "While we started out originally with periodic reports of savings, it was soon found ... to be better to give full credit to production officials,

even though the idea was not of their own initiative but had to be sold to them."

It is partly because of the difficulty of interesting engineers and manufacturers in new developments that they did not have a hand in making that some large organizations which have centralized research laboratories follow the principle of making any new development produced there consist not of a product ready for manufacture, but simply of a typical one embodying principles that can be incorporated into improved products. The actual incorporation of the discovered principles into the product itself is left to the manufacturing organization concerned, but with the privilege of using as much help from the central research laboratory as it may want. This plan accomplishes two things. The one is that the design of the finished product thus fits at once into the manufacturing methods of the company concerned. The other, and perhaps more important, is that, because the engineering organization of the manufacturer has had a hand in developing the product, they consider it as partly their own, and they accordingly take an interest in the product that is much more active than if it came to them as a finished one. The advantages of this system are not all one-sided, by any means, for the staff of the research laboratory perhaps gets as much help out of such a coöperative endeavor as does the engineering staff of the manufacturer. And, in addition, the problem of selling the idea is thereby solved in an admirable manner.

PART III

MEN

THE director of a research laboratory is not like King Richard. He would not give his kingdom for a horse, for he has no need of a horse. But men a research director does need, men of unusual talents, and for the right man he would readily give his kingdom. That is why that great researcher and director of the laboratory of the Royal Institution, Sir Humphry Davy, said that the greatest of all his discoveries was Michael Faraday.

A research laboratory being of course no better than the staff which mans it, it is appropriate that the Third Part of this work—and the Fourth Part of it as well—should tell something about the characteristics of the men who make up a good research staff.

CHAPTER XIII

MEN OF MANY TALENTS

NEWSPAPER men, who pry into the doings of people, have long been dubbed "The Fourth Estate." Those who delve into the doings of things, and who are therefore our explorers up-to-date, have accordingly been named by Arthur D. Little "The Fifth Estate." "This Fifth Estate," says Dr. Little, "is composed of those having the simplicity to wonder, the ability to question, the power to generalize, the capacity to apply. . . . It is their seeing eye that discloses, as Carlyle said, 'the inner harmony of things; what Nature meant.' " Naturally such men, like great explorers or pioneers in any field, are rare. They are those whose love of new knowledge is so great that they rejoice as much over the finding of some new truth about nature as the conventional explorer exulted over the discovery of a new continent.

It is fortunate, though, that not every one who engages in exploration of the up-to-date kind need have the rare qualities of the pioneer, for each generation has relatively few real pioneers. Nor does every one who works at research need to be the kind of man who is popularly thought of as a scientist. There are places of usefulness in research for men of many tal-

ents. The efforts of those who are gifted with a wealth of the pioneering spirit are most effective when assisted by men who, although lacking the rare qualities of the pioneer, are good investigators of the validity or the usefulness of new ideas; and when these investigators, in turn, are supplemented by helpers of various kinds. There are probably a great many more men, and men of more diverse talents, in the ranks of the army led by the leaders in research to-day than those who made up the roving bands of geographic explorers at any time in the past.

Those who helped Columbus discover America, aside from Queen Isabella with her jewels, comprised the carpenters and other artisans who built and fitted out his ships, the instrument makers who made his compass and other navigation equipment, the sailors, the soldiers, the cooks, and every other kind of toiler who helped the wavering boats to push westward. They were not all pioneers, nor were they all explorers in the accepted sense. Some of those who went along were cowards, or at least less brave than Columbus. But every one of them contributed something to the voyage of Columbus, and so to the discovery of the New World.

It is pretty much the same with the kind of exploration that is done in research laboratories. The people employed there, and who make contributions of one kind or another to the progress of any one of the investigations that are under way, may consist of machin-

ists, toolmakers, instrument makers, pattern makers, molders, draughtsmen, electricians, plumbers, sheet metal workers, glass-blowers, engine men, chemists, physicists, bacteriologists, mathematicians, and library workers, together with business managers and supervisors, accountants and stenographers, as well as laboratory helpers of various kinds, including even the wash-boy. It sometimes happens that a most essential idea is contributed to a new development by some mere workman in doing what he could on his part to boost it on its way. Ideas are not respecters of persons. As H. L. Horning has said, the answer to a research problem sometimes emerges from under the dirtiest of hats. There is an old Breton proverb which says, "It takes nine tailors to make a man." To do a good job of research may take nine times nine, only a limited portion of whom could be classed as "scientists."

Even the man behind the information desk at the laboratory entrance, by keeping the various workers from unnecessary disturbance, helps in prosecuting research. Another man who can be most helpful in carrying on research is the purchasing agent or procurer of supplies. A wide variety of materials is needed by a research laboratory, some of them from unusual sources, and many are wanted that can not be purchased anywhere at all. But diligent search sometimes reveals unexpected sources of supply that will save a great deal of work. Furthermore, most things are needed in such small amounts by a research laboratory

that it is usually a very poor customer. So it is that a tactful and resourceful purchasing agent can be of great help to the staff of a research laboratory.

In the case of industrial research, it happens that some of the men who can help—or hinder—most are not on the staff of the research laboratory at all. They are the men who constitute the management of the business which supports the research laboratory. If the management does not recognize in a general way what it needs to find out, says Charles F. Kettering, then there is little use for them to support a research laboratory; for, no matter how many discoveries are made there, none of them can become operative unless those who are responsible for putting things into effect are properly appreciative of their importance and usefulness. There have accordingly been instances of the failure of research in industry because of the lack of appreciation of the essential part the management of a business must play in its research activities.

It was Thomas Midgley, Jr., who suggested that, after all, research is often more of an art than a science. Consisting as it does of delving into the unknown, and often of floundering around in the darkness, there is plenty of room for the application of the art of discovering things, as distinguished from the systematic or scientific method of doing so. This is why so many of the great discoveries have in the past been made by men who would not ordinarily be thought of as having the qualifications to do so. Thus, it was a brewer

who discovered osmosis, and another brewer who measured the mechanical equivalent of heat. That heat is molecular motion was discovered by a soldier, Count Rumford. The celluloid film was invented by a preacher, Hannibal Goodwin. Antony Leeuwenhoek, the first man ever to see microbes, was a small-town dry goods merchant and a janitor. And in *The Outline of History*, H. G. Wells said that "It is worth noting that nearly all the great inventors in England during the eighteenth century were working men, that inventions proceeded from the workshop, and not from the laboratory."

But, in spite of all this, it is of the highest importance for a research laboratory to have a few men with the originality, the vision, and the courage of the true pioneer. So far as a research laboratory is concerned, such men come in the catalyzing group that William James called "ferments." Without the fermenting influence of men who are gifted with some measure of the true pioneering spirit, not much in the way of outstanding new developments can be hoped for, for not often does the ordinary man have either the vision or the courage necessary to make real steps forward. "Next to having no research department at all," says Frank B. Jewett, "I can think of but one greater evil, viz., that of having a research department staffed by second-rate and essentially incompetent men."

In *Creative Chemistry* Edwin E. Slosson told a story

that illustrates how talent of different kinds is needed to make a success of a new development of any considerable magnitude. In 1840, the German chemist, Justus von Liebig, showed that it should be possible to maintain soil fertility by the application of certain essential chemicals, which he named. It was accordingly assumed that all that need be done was to analyze the soil and the harvested product, and from the results to figure out by simple book-keeping methods just how much of each ingredient would have to be restored to the soil after each crop. But practical farmers found that it did not work out that way at all. So they sneered at the professors, and whenever Liebig was mentioned they, as Dr. Slosson said, "irreverently transposed the syllables of the name." When chemists then began to delve deeper into the subject, they found that they must deal with colloids and with damp, unpleasant, gummy bodies that had always before been fought shy of by chemists, because they would not do the things a chemist loved to have the matter he worked with do, crystallize and filter. So the chemist had to call in the aid of the physicist and the biologist. And all three of them, together with practical farmers, had their hands full before they finally succeeded in solving the problems of fertilizing the soil by chemical means.

It is because he realizes that a new development of any consequence must consist of the combined contributions of many workers of various talents, rather than

being the exclusive product of one brilliant man, that the research worker, as Valentine Karlyn said in the *New York Times*, "spurns such flattering designations as 'wonder-worker,' 'wizard,' or 'magician.'" Thus, W. D. Coolidge said of the great contribution of ductile tungsten wire, which he and his associates made to the lighting industry and to the world, "We see in it rather the fruit of hard work than of genius—unless hard work and genius are one and the same thing."

CHAPTER XIV

TRAINING

THAT pioneers are born, not made, is a conclusion for which evidence can easily be found in the records. Priestley, one of the greatest chemical discoverers who ever lived, was a preacher. So was Charles Butler, who found out so much of what is known about bees, and who corrected the long belief, as expressed by Aristotle and by Shakespeare, that the king-bee rules the hive. Mendel also, the discoverer of the laws of inheritance, was a Moravian monk and an abbot. Pasteur, the world's most useful bacteriologist, was trained as a chemist. Before he began his long life of research, Thomas A. Edison was a train "butcher," a telegraph operator, and a self-made chemist. Alexander Graham Bell, the first telephone man, was a teacher of the deaf and dumb. Sidney G. Thomas, who discovered the basic Bessemer process of making steel, was a clerk in a police court. Charles Darwin never went to college, and neither Wilbur nor Orville Wright finished high school. Dr. A. A. Michelson first made his classical measurement of the velocity of light in preparation for a classroom demonstration at Annapolis, and with only the training of a naval officer. Before he became the great astronomer that he was, Sir William Herschel was an

army musician, a deserter, a music teacher, a pipe-organ player, and an actor; and his sister Caroline, who aided him so mightily, was uneducated and knew little besides housework. Even the great Michael Faraday once wrote of his early education that it "was of the most ordinary description consisting of little more than the rudiments of reading, writing, and arithmetic, at a common day-school." So it is not at all strange that the matter of making scientific discoveries sometimes appears to be, as Paul de Kruif said about microbe hunting, "A queer humpty-dumpty business."

But in reality it is no more peculiar that the men mentioned should have contributed so largely to science and industry along lines apparently outside their natural fields than that men have made great contributions in other fields of human endeavor without apparently having been trained for it. John Bunyan was a tinker, and an imprisoned tinker at that; but he wrote one of the world's great books. Robert Burns whose training was that of a farmer turned out to be a much better poet than farmer, but some of his best poetry was written about life on the farm. The great post-war German statesman, Dr. Gustav Stresemann, won the Ph.D. degree which he had, not in politics, but on the development of the Berlin market for bottled beer.

From what has just been said it should not be inferred, however, that for the undertaking of research specific training is not necessary. It is true that to a man of genius in any field the character of his early train-

ing and experience is sometimes not of paramount importance. And practically all the men mentioned could perhaps be classified as geniuses. But, for the most part, the experience of such men does not apply to the great majority of us, or at least it does not apply to us to the same degree as to the man of genius. With the most of us it is important to get proper preparation for whatever kind of a job we have to do. Usually the man who undertakes research with inadequate preparation is like the mechanic who tries to do his work with ten-cent-store tools.

In one respect education for research is like education in general—there is difference of opinion about what it should consist of. And, because the people who take the various courses of training differ so widely in their personal characteristics and capabilities, there is justification for some of the differences of opinion that exist. And here it may as well be said that the remarks which follow on how to train for research should be taken as mildly suggestive only. They will purposely be made rather general. Thus it is not intended to say whether or not a Ph.D. degree is a requisite handle for a research worker's name to have. It may be necessary, and again it may not. But surely desirable is the training needed to fulfil the requirements for such a degree.

In one important respect the best kind of an education for research differs somewhat from education of the conventional form. Since research consists of a

search for new knowledge, the training that can do a prospective research worker most good is not so much that which increases his familiarity with present fields of knowledge as that which shows him how to acquire new knowledge, or how to expand further the boundaries of some field of knowledge. But still, of course, it is important to know, or at least to be able to find out, what the scope and the boundaries of present knowledge are.

The first step in preparing for research is therefore to lay the best possible fundamental foundation, the more fundamental the better. This foundation should be laid in the *principles*, not so much in the *facts*, that apply to what is known about matter and energy. This means that a person who plans to do research of the conventional kind should be as well grounded as possible in the fundamentals of the general sciences, such as physics, chemistry, and mathematics.

It is thus *education* in the fundamentals of science, not just *instruction* in them that the prospective research worker needs. "Education," in the words of Sir William J. Pope, "consists in expanding the powers and resources of the mind, whilst instruction consists in filling it with information." Professor Neil E. Gordon of Johns Hopkins University has expressed this same idea by saying that education is not the process of "stuffing in," but rather that of "leading-out." Certain it is that no mere fund of information, however broad it may be, can qualify any one to be

a searcher for new knowledge. Education for research, like true education in general, must train a man to think. Thomas A. Edison considered thinking so essential that, when it was once proposed to relieve him of his deafness, he declined on the ground that his deafness helped him to think; and, said Edison, "I want to do a lot more thinking before I die."

In the light of what has just been said, it is doubtful whether a prospective research worker should secure a highly specialized training to fit him to do one specific thing, unless he wants to be no more than a mere helper of some particular kind. For one who plans to take the initiative in projects of research, each of which is likely to be different, such a specialized training is too narrowing. Nevertheless, if a worker expects to operate mostly in one general field, such as that of chemistry, he should be particularly careful to lay a broad foundation in that subject. "A man may be asked to work in one field to-day and in quite a different one to-morrow," said W. D. Coolidge. "And for this . . . reason it is very necessary that the research worker himself should be as well grounded as possible in fundamentals, rather than specialized too closely in some one limited field."

Frank B. Jewett, of the Bell Telephone Laboratories, speaking of the matter of specific training for a particular line of endeavor, said this: "Some years ago . . . the scientific schools were inclined to ask us to arrange their courses. Obviously, this was a mistake.

We could tell them only of the present state of the telephone art. Any courses suggested by us might be out of date by the time the student graduated, for the science of telephony is like other sciences in that progress is continually being made. It is the task of the university to sift the raw material that comes to it, to encourage the talented student, to drive home the fundamentals, to create the proper outlook toward the field of science."

Another thing that appears to be important in preparing to do research is to learn how to use the library to good advantage, for the library can be of the utmost help in research. Not to know how to use it with facility is, accordingly, a most serious handicap. And, because science knows no national boundaries, it is important to be able to read the writings of men of other nations. Hence the obtaining of a reading knowledge of French and German in addition to English, as the three principal languages in the world of science, is also an important item in the education of a research worker.

And then there is the matter of training in the speaking and writing of the English language. The ability to tell others about the results obtained in research and about why certain investigations are or should be carried out is one of the most vital things of all. The research worker who can not give an understandable and forceful verbal account of his work, or who can not write a lucid report of it, labors under a great handicap.

And, unfortunately, this is a handicap that many research men do have, for technical men are notoriously lacking in powers of effective expression.

It is not the gift of impeccable English that is meant here at all, but rather the ability to give a brief and precise description, and in particular one which is expressed not in the language of the writer or speaker, but in that of the reader or the listener. Old and fundamental, but nevertheless often violated, is the rule that ideas should be expressed in terms that the listener can understand. Thus, the apostle Paul, who was unusually successful at talking the other fellow's language, prided himself on being "all things to all men." To the Corinthians he wrote, "Except ye utter by the tongue words easy to be understood, how shall it be known what is spoken?" Professor D. W. Thompson told in *Nature* about a paper which was once read before the Royal Society of Edinburgh by a certain young author. "It was," he said, "rather a dull paper, on the 'Thermal Influence of Forests.'... It made a good show of meteorological learning.... Its style was technical and scientific." With striking climax Professor Thompson then told how the author of that paper "did better, much better later on, when he wrote a book called *Treasure Island!*"

Too many research workers violate the rule that ideas should always be put into the language of those addressed. In writing reports to be read by men not familiar with technical language, as often needs to be

done in research, they find it impossible to drop into common parlance, but continue to write somewhat as follows:

The calorific value of the compound C_2H_5OH is equal to 326 kilogram calories per gram mol.

How much better in such an instance to say:

The heat produced in burning one pound of pure ethyl alcohol is 12,800 B.t.u. This is about the same as the heating value of a pound of good coal.

Closely related to the ability to tell others about one's work is that of keeping comprehensive, accurate, understandable, and legal records. It is particularly bad when the records kept by a research worker are not sufficient to show what experiments he made, nor to establish the legal priority of discoveries that may have resulted from them. But, unfortunately, many instances have occurred where this has been true. Even such an important item as the date on which the experiment was made is sometimes omitted.

There are of course other things besides those mentioned that a research worker should know. Thus, for instance, if he expects to do research in industry, he should have some understanding of the principles of economics. He needs as well to know something of patent law and practice. Also his education should not be so narrow that he will not have, in the words of W. C. Geer and Charles M. A. Stine, "at least a bowing acquaintance with the world of literature, the fine

arts, and much of what goes to make life interesting and worth living."

The tabulation below is a composite of how, in a survey conducted by Johns Hopkins University, the heads of the chemistry departments of ninety-eight schools rated the characteristics of prospective research men who were candidates for graduate training in experimental chemistry:

	<i>Average Weight, Per cent</i>
<i>College Standing:</i> Marks in chemistry, physics, mathematics, and English.....	8.72
<i>Book Ability:</i> Capacity to learn from books.....	8.21
<i>Creative Ability:</i> Initiative, imagination, originality, resourcefulness, ability to see and solve problems	14.04
<i>Intellectual Honesty:</i> Dependability, reliability of work	11.84
<i>Perseverance:</i> Application, persistency.....	9.16
<i>Faculty of Observation:</i> Power to observe and record results accurately	9.22
<i>Enthusiasm:</i> Energy, interest put into work, enterprise	9.15
<i>Conduct:</i> Coöperative attitude toward others, consideration for others.....	8.36
<i>Character</i>	11.84
<i>Health</i>	9.46
	<hr/>
	100.00

In spite of the fact that it is quite an essential thing to have, the importance of book learning should not be over-rated. It represents only one of the legs that ability to do research works on. And research ability

is a centipede, or at least it uses several legs, every one of which is needed. Andrew Carnegie was once asked which of the three he thought the most important factor in industry: labor, capital, or brains. His response was the question, "Which is the most important leg of a three-legged stool?"

Note that, from the above, College Standing and Book Ability are together rated at only about 17 per cent of the necessary qualifications of a prospective research worker, not as much as Perseverance and the Faculty of Observation. It is thus that the cultivation of faculties such as the latter ones form a most essential part of a proper training for research. So far as book learning was concerned, John A. Mathews, one of the greatest of metallurgists, took most of his university work not in metallurgy but in organic chemistry. Thomas Midgley, Jr., also, one of the most productive men living, in the field of chemistry, had his university training and early industrial experience chiefly in mechanical engineering.

Because of the importance of cultivating other items in his character, aside from those ordinarily thought of as constituting an education in science, the prospective research worker needs in his preparation to make the distinction involved between the two expressions: "learning how to swim" and "learning to swim." In research, just as in other lines of endeavor, there is such a thing as knowing about *how* it is done, without actually being able to *do* it.

So it is that, at some stage in the training for research, one of the valuable things to do is to attach one's self to a master of research, to work with him and for him, and so to learn how to *do* research—or at least some of the ways to do it. This is really a form of apprenticeship. But it is a logical and practical thing to do, because, as has already been suggested, research consists so largely of an art as distinguished from a science. That was the way Michael Faraday learned to do research. He apprenticed himself to the great Sir Humphry Davy. Faraday even served Sir Humphry as a servant or valet when occasion demanded, as well as a helper in his experiments. And in the light of the relative weights given to the various qualities of a research worker in the above table, it is not surprising that, following so much intimate association with a master of research, Faraday succeeded so well in spite of the very limited education in science with which he started. In a similar manner, Sir William Crookes, after having left school at the age of fifteen, apprenticed himself to the great chemist, August Wilhelm von Hofmann. What he learned from Hofmann seems to have been principally in methods of experimentation, for the subject of organic chemistry to which Hofmann devoted his energies appears to have had but little attraction for Crookes.

This scheme of apprenticeship is, to some extent, the system that is followed in our universities to-day by men working for advanced degrees, a portion of the

requirements for which is the making of an original investigation of some kind. A candidate for an advanced degree selects some member of the faculty with whom to do his thesis work, and under his direction, or with his association, an original investigation along some line is undertaken.

Because the association that one has in the actual doing of research can thus constitute a most essential part of his training, it is important to attach one's self to a man from whom the best methods can be learned, and who spends enough time around the laboratory to allow the student really to learn from him by personal association. The choice of whom to tie up with thus requires some care, for not every teacher who does research is well qualified for it. It is an unfortunate fact that some do research only because in many schools the publication of the results of a certain amount of original investigation has been made a requisite for advancement.

Whatever the prospective research worker's education may consist of, it should not be of such a character as to take from him his natural inquisitiveness and enthusiasm for experimentation, and to substitute for it the hampering belief that some things are impossible because they can not now be done. A certain amount of ignorance is preferable to that. One of the chief reasons why Michael Faraday turned out to be such a great and useful experimenter was because he never lost that spirit of experimentation which once as

a youth made him put his head through the railing that separated the entrance to his house from that of the next one and consider which side of the railing he was then on.

That particular experiment, by the way, was one of those which turned out badly; for just then some one unexpectedly opened the door on the side where Faraday's head was, causing him to draw back hastily and strike his nose so hard that it bled. Thus did Faraday give some of his blood in the cause of science, or at least in the effort to satisfy that inquisitiveness of his which later on made him such a useful man.

What has been said about training thus far is concerned chiefly with the character of the preparation that should *precede* the undertaking of research as a vocation. But it is important for every prospective and practicing research worker to realize that training for research should not cease when he leaves school to take a job in a research laboratory. Charles F. Kettering threw his diploma away when he graduated from the university for fear that it might make him feel that his education was finished. Training in college or university is necessarily so brief that it can not do a great deal more than lay a firm foundation upon which to build a structure of outstanding capability for research. And thus it is highly important that care be taken to see that the foundation which is laid there be thoroughly and solidly grounded upon the fundamentals of the basic sciences. The broader and the sol-

ider that foundation is laid the bigger and the stabler the structure of education and experience that can later be erected upon it, of course.

In continuing his education through life it is not enough for a research worker simply to sit back and accept merely such information as happens to come his way. On the contrary, he must put forth a diligent effort to improve himself in all matters pertaining to research, and particularly in those which lie within his chosen field of endeavor. One of the ways of doing that is to keep well posted on what other men are doing in his own field and in fields bordering his. Doing that demands much reading and study, together with whatever is possible in the way of personal contact with other workers. This suggests that it is therefore a good idea for a research worker to be a member of some of the better professional societies which are composed of men working along lines that are similar or allied to his own, and then to establish personal contact and acquaintanceship with fellow members by attending such meetings of the societies as it is possible for him to attend. But, whatever the means a research worker may find best for keeping his education from stopping, it is important that he do keep on with his training just as long as he tries to do research.

CHAPTER XV

RECRUITING

SELECTING the members of the staff of an organized research laboratory is not greatly different in method and in difficulties from building up a baseball team. In both, men of various kinds of ability are needed to play the different positions, but each man must know the game and be able to play it well. Outstanding individual ability is much sought after in both. Such ability is not worth much in either case, though, unless its possessor is willing and able to subordinate it to good team play. Coöperative ability is thus just as much an essential qualification of a research worker as it is of a baseball player. "Prima donnas" sometimes make about as much trouble for directors of research as they do for baseball managers.

It is very necessary that the men "working in an industrial laboratory should be able to coöperate well with other people," said W. D. Coolidge in a recent address. "In the early days, our laboratory was frequently referred to as the bears' cage, and an important part of the director's job was to keep the bears from fighting. But we found that it did not pay to keep a man, no matter how good he was, if he could not get along well with the rest of the staff."

Neither the baseball team nor the research laboratory can depend altogether upon local talent for its personnel. Both send out scouts and scour the whole country for the best in the way of talent that can be found and induced to join up with them, and the research laboratory even goes to foreign countries for special talent if occasion demands. When the new man is chosen young, as usually happens both in research and in baseball, it is pretty much of a gamble whether he will be good enough to become a major, or whether failure to measure up to what is required for such success will relegate him to the minors.

With regard to the desirability of trying to search out men who might become outstanding geniuses at research, however, C. E. K. Mees of the Eastman Kodak Company has said: "We have no right to assume that we can obtain men who are geniuses; all we have a right to assume is that we can obtain at a fair rate of recompense, well-trained, average men having a taste for research and a certain ability for investigation."

The trouble with trying to pick out a genius, especially before he has proved himself, is that it is hard to tell whether such a man is really unique enough to be a genius, or whether he merely departs from the conventional in some altogether useless respects. About as well try to pick the particular oyster that has a large pearl in it, as try to pick a genius out of the rest of humanity. "A boy that frowns at umpires ain't

necessarily a Babe Ruth," says Chic Sales. And any one who has already demonstrated himself to be a real genius in some field of research is not likely to be won to a new connection, for research workers are not usually shifted from laboratory to laboratory as baseball players go from one team to another by trade or sale. Anyway, so far as the cause of research in general is concerned, such a shifting is, in the words of Frank B. Jewett, "a mere shuffling of the cards in the deck and in some cases is ethically objectionable."

Fortunately, most of the qualities that fit a person for the doing of research are the same as those which make for success in other fields of endeavor. These qualities are such fundamental ones as honesty, straight thinking, hard work, patience, persistence, courage, resourcefulness, imagination, modesty, enthusiasm, and common sense. Two other qualities that are particularly useful to the research worker are inquisitiveness (not about people, of course, but about things) and power of observation. The possession of all these qualities to a properly balanced degree, coupled with a suitable training in the fundamentals of science and a liking for experimentation, is good assurance that a man can be reasonably successful at research. But, as for picking out a great genius, no dependable rules can be given for doing that.

"You have got to get a man who is 'sold' on research before you can hope to get anywhere in research work," advises Charles F. Kettering.... "What you

want is a man with the proper research spirit.... You do not want the type of man who is doing his work from day to day, the kind that goes to the theater at night and ... plays golf in the afternoon.... What you need most is somebody bitten by the research bug."

So hard is it to find men of outstanding capabilities as searchers into the unknown that Sir Humphry Davy could well say, as mentioned before, that the greatest of all his discoveries—and Davy made many important discoveries—was Michael Faraday. Davy himself was likewise one of the great discoveries of his own predecessor in the directorship of the Laboratory of the Royal Institution, Benjamin Thompson or Count Rumford. One of the biographers of Torbern Olof Bergman, the great Swedish chemist and mineralogist, said that "the greatest of Bergman's discoveries was the discovery of Scheele." Perhaps no more prolific chemical discoverer than Karl Wilhelm Scheele ever lived. Charles P. Steinmetz was one of the greatest discoveries of E. W. Rice, Jr., who directed the engineering of the General Electric Company during the years while the foundation of its success was being laid. It is probably true of every one who directs research that his greatest discoveries are some of the men who work with him or for him.

In respect to the proper way to go about searching for research men, it is best perhaps to give here the advice of a man who has done a great deal of such searching, and who, as events have proven, has had

somewhat more than average success at it—Frank B. Jewett of the Bell Telephone Laboratories. Speaking before the American Association for the Advancement of Science at the end of 1928, Dr. Jewett gave three rules for conducting such a search. The first was that the search should be made among *young* men. The second was that the best places to search are the universities and scientific schools. The third was that the judgment of men in the academic world who have had first-hand opportunity to know and appraise the men under consideration for considerable lengths of time should be given large weight.

“Summed up,” said Dr. Jewett, “I should say that in attempting to select young men who in later life will be successful in industrial research, a primary requisite is to come to know the wise men in our college, university and technical school faculties whose judgment applied to the young men they have instructed makes them a more efficient sieve than any casual outsider can be. True, they may not be able to tell you that ‘X’ or ‘Y’ is suitable for your particular situation—that is a matter which you alone are in the best position to judge. They should, however, be able to give you substantial advice, not only as to character but as to the reasonable chance that the youthful evidences of ability are the early fruits of a substantial continuing harvest and not merely the exotic flowering of a hot-air plant.”

Evidently, in the matter of recruiting research men,

Doctor Jewett does not believe very strongly in the value of snap judgments or of sizing men up by appearances. In this respect, he was not like Ronald Ross when Ross was looking for assistants to help him in his search in India for the malaria microbe. The first of these, Mahomed Bux, wrote Paul de Kruif, "Ross hired because he had the appearance of a scoundrel, and (said Ross) scoundrels are much more likely to be intelligent."

F. O. Clements, technical director of the General Motors Research Division, tells that several years ago when he was with the National Cash Register Company he used to try to "size up" the star salesmen on the occasions of their annual visits to the factory to see if he could discover what it was that made them outstanding members of the famous "Hundred Point Club." But he could see no visible mark of sales ability that was common to every one. For some time the very best salesman of all was an insignificant looking fellow who was so crippled that he had to go on crutches all the time. Yet that man's record of sales topped all of them. He sold a cash register every single day on the average. The things about those star salesmen that made them outstanding—their tact, their persistence, their unusual capabilities for head work and for hard work—could not be seen. And the same applies to research men.

Again, as at the outset, it may be suggested that the selection of a research staff is not unlike the choosing

of a baseball team. And in research, as in baseball, the best place to look is among young men, and among those young men who actually play ball, or who in our case work at research. A university or technical school is practically the only place in which young men of promise have a chance to do research in any organized or consistent way. That is why the best scouts for a research laboratory are the members of the faculties of such schools. Their value as scouts arises from the fact that they have an opportunity to see the men play more than one single game. Instead, they see them at work in the laboratory and in the class room for months. In addition to the opportunities they have to observe young men at work, these faculty members are usually research workers themselves. And so their judgment about the capabilities of the men who come under their observation is likely to be better than that of any one on the outside can possibly be.

All that has been said above applies in a strict manner only to the recruiting of men of technical, engineering, or scientific training. But such men do not constitute the whole of a well-rounded research staff by any means. Of great importance in research are the assisting artisans and the helpers of various kinds whose contributions to the success of any research project are usually of a most essential character. Men like Charley Taylor, that capable mechanic who served the Wright Brothers so faithfully and so well, are as hard to find as they are necessary and valuable. Of such men, there

is no single dependable source. They simply have to be acquired in a gradual way by selection from those who come to work in the research laboratory or at some other place within the organization or outside of it where their talents manifest themselves.

PART IV

QUALIFICATIONS

CONTRARY to the conception of some, a research laboratory is not populated with people queer in looks and in actions. The long-haired eccentric, who represents some people's conception of a "scientist," does not exist in fact. Research workers look and act just like other people, for most of the personal qualities which are requisite to such workers are the same as are essential to success in any calling. There are certain of the common qualities, though, that the research worker needs to have to an especial degree, and some of these are discussed in the following chapters.

CHAPTER XVI

YOUTH

DON'T you have any one but boys on the staff of the research laboratory? I never saw an organization before in which all the men seemed so young." This was what the surprised wife of the business director of one research laboratory said to her husband after having made her first visit to the institution he was the manager of.

But the lady should not have been surprised at all, because among pioneers youth has always been prominent. Lindbergh flew the Atlantic at twenty-five. Columbus was an explorer at the same early age. Livingston plunged into Africa when he was twenty-seven, and Stanley was one year younger when he set out to find him. Alexander the Great had conquered the known world and died by the time he was thirty. Cæsar, Hannibal, Charlemagne, and Napoleon: all were conquerors before thirty. The reason for this lies in the spirit that Charles Kingsley wrote about in one of his poems:

When all the world is young, lad,
And all the trees are green;

Then hey for boot and horse, lad,
And round the world away.

Research in particular is a form of pioneering in which youth has a natural and a conspicuous place. Newton formulated the law of gravitation at twenty-four. James Watt was twenty-three when he began experimenting with steam. Eli Whitney invented the cotton gin at twenty-six. McCormick was only twenty-three when he invented the reaper, and Westinghouse was the same age when he invented the air-brake. At twenty-two Linnæus developed the method of arranging plants by sexes, which formed the basis of the great fame he later achieved. Friedrich Wöhler was only twenty-eight when he founded the science of organic chemistry by finding how to make urea in the laboratory. Perkin discovered the first aniline dye at eighteen. Hall discovered his process for extracting aluminum from bauxite at twenty-two, when only a few months out of college. Edison began his life-long experimentation in his teens, before he was twenty-five he was perfecting new systems of communication, and he invented the incandescent lamp at thirty-two. Irving Langmuir began his great research on the incandescent lamp at twenty-eight. Frank B. Jewett was only twenty-five when he took charge of engineering research for the American Telephone and Telegraph Company and began to build up what has become the largest research laboratory in the country.

But in research, as elsewhere, "Youth is not a time of life—it is a state of mind." It is mental youth—as typified by imagination, optimism, enthusiasm, cour-

age, and capacity for change—that is essential in a successful research worker. That spirit can not be measured in terms of chronological years. The many reverses that are met with in the up-to-date form of exploration called research demand that those who are engaged in it be fortified against discouragement with the buoyant hopes of youth which “turn, like marigolds, toward the sunny side.”

On account of its characteristic mingling of courage and optimism, youth does not know enough not to attempt the “impossible.” The consequence is that it often does the impossible. Charles F. Kettering has said that “A man must have a certain amount of intelligent ignorance to get anywhere with progressive things.” Ignorance of the intelligent kind, which is one of the outstanding characteristics of youth, has certainly been a big asset to the world, because it has helped a great deal toward converting the impossibles of yesterday into the actualities of to-day. “For God’s sake,” wrote Robert Louis Stevenson, “give me the young man who has brains enough to make a fool of himself.”

When Charles F. Kettering was developing the self-starter for the automobile and trying to get it successfully established on the Cadillac car, which was the first to use it, one of his greatest difficulties was to overcome the resistance set up by the expressed opinion of some of the country’s outstanding electrical engineers that to crank an automobile engine with a storage

battery would be a physical impossibility. Because Mr. Kettering was too ignorant to know that, and too stubborn to take the advice of the electrical experts, he went right on and made the self-starter a success, to the great benefit of every one who has driven a car since that time. When Thomas Midgley, Jr., first began to make fluorine-containing compounds in his search for a refrigerating gas that would be both non-toxic and non-inflammable, every one told him that such a thing was out of the question; for is it not well known that compounds of fluorine are very poisonous? But he persisted in spite of them and got fluorine-containing refrigerants that were at once non-inflammable and almost as non-toxic as the very nitrogen in the air.

At the time when Alexander Graham Bell was shouting into a contraption made from a dead man's ear in his struggle toward the first telephone, even those two friends of his, Sanders and Hubbard, who furnished his financial support, thought that he had no chance at all to succeed. But Bell, being so ignorant as to think he could "make iron talk" over an electrified wire, finally found a way to do it. Fortunate it sometimes is that not only are the thoughts of youth, "long, long thoughts," as Longfellow said, but also that they are very stubbornly held on to.

Another outstanding reason why youth has always had a large place in research arises from the situation that the money available for the support of research has never been unlimited in amount. The services of

youth can usually be purchased at prices that are low relative to their quality. This is partly because the young man of scientific or engineering training has not yet made a reputation that will justify any one to risk more than a small investment in his services, and partly because he has not yet acquired a sufficient knowledge of the intricate background of any particular industry to fit him to fill a position of much responsibility in it. The latter is probably one of the things that Bacon meant when in *Of Youth and Age* he said, "Young men are fitter to invent than to judge."

It is both because the buoyant qualities of youth are needed there and because the price of youth is not high relative to the quality of the service it can render that the staff of the research laboratory consists so largely of young men—and sometimes nowadays of young women, as well.

CHAPTER XVII

CURIOSITY

CURIOSITY is said to have killed the cat. Nevertheless, curiosity is a trait which every effective investigator has to have a great deal of. He must, in the words of Gilbert K. Chesterton, be "inspired by sheer, shameless, round-eyed, gaping, goggling, curiosity." But, as has already been suggested, the necessary inquisitiveness of the research worker is not about what his neighbors are doing. Rather it is about what *things* can be expected to do. In that respect his curiosity must be of a very persistent kind. It should in fact be as undying as that of the inquisitive little boy who, when he was warned about the effect of curiosity upon the cat, promptly asked what it was that the cat wanted to know.

This greater curiosity about things than about people, which has to characterize the scientific explorer to some degree, is such an unusual human quality that Anatole France, who perhaps knew no scientific worker in an intimate way, defined a scientist as "one who is interested in something that is fundamentally uninteresting." But, of course, those things which fail to interest a novelist are not necessarily without interest to other people. And, in respect to scientific things, it is lucky that they are not.

The superior interest in things that distinguishes some people comes out in a story that A. C. Langmuir once told about his younger and now more famous brother, Irving Langmuir. When A. C. Langmuir was twenty-four and just engaged to be married, he was telling Irving, who was then only fourteen but already intensely interested in the things of nature, about the newly discovered gas, argon. It was argon which several years later Irving found a valuable use for in filling the bulbs of incandescent electric lamps. "I was telling my brother what I knew about this discovery," related A. C. Langmuir, "and then changed the subject to what was uppermost in my mind, saying, 'Irving, do you know that I am going to marry Alice Dean?' His reply was 'Oh,' a pause, and then, 'But Arthur, you were telling me about argon—'"

Success in research depends a great deal upon how easily the curiosity of an investigator is satisfied, or upon what kind of answers to his questions he is willing to accept. That is to say, it depends upon how accurate and how fundamental or basic are the facts that the questioner insists upon getting. A smattering of information, even though it be exact in so far as it goes, is not usually enough.

In his book, *In Darkest Africa*, Henry M. Stanley told how in 1888, during one of his journeys through the fever-infested areas of the Dark Continent, he reached the conclusion that the "malarial influence" was "screened" out of the air under certain conditions.

And away back there Stanley suggested that "a respirator attached to a veil, or a face screen of muslin," might "assist in mitigating malarious effects." He recorded also that Emin Pasha informed him "that he always took a mosquito curtain with him, as he believed that it was an excellent protector against miasmatic exhalations of the night." Note particularly the references to "mosquito curtain" and to a "face screen of muslin." If Stanley had only found out enough to substitute the exact term "mosquito" for "malarial influence" or for "miasmatic exhalations of the night" he and his party could have been entirely free from the scourge of malaria all the while they were in the Dark Continent. That one essential fact he did not find out, however; and it was not until ten years later that the persistent researches of Ronald Ross and Giovanni Grassi yielded exact information that malaria is carried by certain mosquitoes.

The name anopheles mosquito or *Anopheles claviger* is not as mystical as "miasmatic exhalations of the night," to be sure, but it is exact and specific, which is a necessary characteristic of the information upon which scientific advances are made, or by means of which practical problems are solved. It may well be that there is another reason besides romanticism for the figurative language of the Oriental. And that is that his store of hard facts has sometimes not permitted him to speak in terms that are more exact than poetical.

In this matter of insisting upon a fundamental an-

swer to his questions, the research worker has to be a bit unnatural. That is to say, he has to run counter to some of his inherent human traits. Being satisfied with a superficial answer is altogether natural to people. It is likely that every one when traveling has had the experience of coming to a stream of water and wondering what river it was. And, if some one happened to be able to tell him that it was the Androscoggin or the Scuppernong, his curiosity was often satisfied, even though the name meant nothing at all to him beyond a mere name. Curiosity of such a superficial form can be of very little use in a research laboratory, for there an answer is useful in proportion to how exact and how comprehensive it is.

CHAPTER XVIII

IMAGINATION

IT was a theory that led Columbus to discover America, and a theory that was wrong. Columbus was looking for India, and he imagined that he would soon come to it by sailing westward, where instead he found the fringes of America. But so firmly did he believe in his theory that he thought the island on which he had planted the flame-colored banner of Spain really was a part of India. The very names, Indians and West Indies, have come down to us as evidence of the continued belief of Columbus in the theory that drove him westward across the "Sea of Darkness."

The importance of Columbus's theory about a short westward route to India was that, had it not been for the theory and his firm belief in it, the three frail ships would never have put out from Palos, nor gone as far as they did. His incorrect theory that India lay but a short distance to the west of Spain was the thing that gave Columbus the stimulus to set sail on a hazardous voyage, and the confidence to continue it in the face of the growing alarm of members of his crew until he had made a much greater discovery than was anticipated by his theory, if it had been right.

In research, theory has even a more important place

than it had in conventional exploration. "Theory alone can bring forth and develop the spirit of invention," said Pasteur. So it is that, if a research worker is to make progress, he must do a certain amount of theorizing, which in common parlance is to use his imagination. A theory—which is simply a formulation of the opinions or the speculations of an investigator—has its principal use as a stimulus to and as a director of further experimentation. A theory which is so plausible as to be accepted in lieu of experiment is a bad thing for research. It may function then, as some one has said, like hobbles on a horse to make a pacer out of him, when by nature he may be a trotter.

In order to serve its useful purpose of stimulating experimentation, a theory need not necessarily be right. Consisting so largely of speculation as it usually does, a theory is easily revised whenever the finding of new facts justifies it. A correct theory is more desirable than an incorrect one, to be sure. But, provided it serves the useful purpose of stimulating further experimentation, even an incorrect theory is better than none at all. It is perhaps an open question whether as many important discoveries have not been made through research stimulated and directed by wrong theories as by right ones. It is often after a discovery has been made, not before, that the correct theory of it is formulated. Even "in the most successful instances," said Michael Faraday, "not a tenth of the suggestions, the hopes, the wishes, the preliminary conclusions have been realized."

It was an incorrect theory that led directly to the discovery of the first anti-knock compound. Beginning the study of knock in engines, Charles F. Kettering and Thomas Midgley, Jr., observed that an engine which would run quite smoothly on gasoline would usually knock violently on kerosene. They reasoned that the chief difference between a gasoline and a kerosene, so long as both came from the same source, was in respect to ease of vaporization. They supposed that gasoline vaporized completely as it entered the combustion chamber, forming a homogeneous mixture with air, which then burned smoothly and uniformly. But kerosene, being less volatile, they supposed to persist in part as minute droplets, even during a portion of the combustion period. They then imagined that at the point where the piston began to recede and release the pressure somewhat, under the high temperature present also at that time, all the globules vaporized at once and burned instantaneously. It was that instantaneous combustion, they thought, that caused the engine to knock. On this basis they reasoned that if the droplets were to be dyed some dark color—like the rusty leaves of the trailing arbutus, which blooms in the spring even before the snow has gone—they would then absorb more radiant energy and so should be vaporized soon enough to prevent the trouble of knock.

It happened that when Kettering and Midgley tried to test out this theory no oil-soluble dyes were to be had. So, as a compromise, they used iodine to impart

the desired reddish color to the kerosene. When the kerosene that had been made red with iodine was run in the engine, the knock did fade out completely and a great discovery had been made. That discovery, which had thus been secured directly on the basis of the theory previously formulated, appeared to be in perfect accord with the speculation that had led the investigators to it. It was soon found, however, that when the kerosene was colored red with a true dye, instead of with iodine, then the knock was not reduced, no matter how deep the color was made. Evidently it was some property of iodine other than its color that had eliminated the knock.

But this theory, although quickly demolished, had been a good theory; or at least it had been a valuable one, for out of it had come the important discovery that the natural tendency of a fuel to knock could be overcome, and that by a very small admixture of iodine. It was thus that an altogether incorrect theory led to the discovery of the first one of the large group of anti-knock compounds, one of which, lead tetraethyl, is the active constituent of ethyl gasoline.

Emil Behring was patiently testing out the theory that diphtheria might be cured with some chemical, such as iodine trichloride, when he discovered that an antitoxin for the poison of diphtheria could be made from the blood of an animal that had recovered from diphtheria. Ronald Ross was working on the theory that malaria is contracted by drinking water contami-

nated with malaria germs from dead mosquitoes having fallen into it, when he stumbled on to the fact that the germs of malaria get into the system from the bite of the living mosquito. "Never," said Paul de Kruif, "has there been a finer instance of wrong theories leading a microbe hunter to unsuspected facts."

Of the long succession of theories about the constitution of matter, many have had to be discarded and others radically revised. But every one of them has helped along in the steady march toward an understanding of what the constitution of matter really is. After Roentgen had discovered X-rays, Poincaré, the French mathematician, imagined that the explanation of phosphorescence might lie in X-rays. The French physicist Becquerel, whose father happened to have made a study of phosphorescence, decided to test out the hypothesis of Poincaré. For his experiments he utilized the same set of salts which his father had previously used in his studies of phosphorescence. The result of Becquerel's investigation was not to establish any connection between X-rays and phosphorescence. But, while experimenting with compounds of uranium, he did discover radioactivity, and thereby paved the way for Monsieur and Madame Curie to find radium.

"There are only two methods of research," says Wilder D. Bancroft. The one Dr. Bancroft calls the Baconian, or that in which the investigator accumulates data until the theoretical interpretation of the data becomes obvious to him. The other he calls the Aristo-

telian, or that in which the investigator first gets a theory or in which he imagines in what direction the solution is most likely to lie, and then sets out to test the hypothesis by experiment. The latter of the two is the one which Dr. Bancroft considers the better.

I am going to say [explains Dr. Bancroft] that those who accumulate facts are accumulators and those who start early with working hypotheses are guessers. . . . I am using the word guess as it is used in New England and not as it is used in Central New York. I do not mean the result of pure chance, such as flipping a coin or deciding whether to finesse on the right or the left when the bidding has shown nothing. I mean by guessing making the shrewdest judgment one can from facts which are not entirely adequate. Guess is the word used by Newton in the same sense, so it has been in good use for several centuries.

The theories of scientific investigators represent the same productive use of imagination as that which is so essential to the artist or the poet. Theories are the product of the mind's eye. Without imagination, a research worker would often be too near-sighted to proceed fast enough or directly enough toward his objective, which is naturally obscured by the haze that he has not yet been able to penetrate, except by means of his mind's eye. "I have a hunch that it would be a good thing for science," said Charles P. Steinmetz, "if it were a little more chummy with dreams than it is. Dreams are but a form of imagination, and without imagination, science would be nothing more than a set

of formulas. Unfortunately it is often nothing more than that now."

One manifestation of imagination that is particularly essential in research, and that therefore should not fail to be mentioned here, takes the form of the practical quality called creative ability. This mental ability to create something new involves more than mere imagination alone. It involves also the initiative and the resourcefulness needed for reducing a product of the imagination to actual realization. "Initiative," as Arthur D. Little said in "The Fifth Estate," "is one of the rarest mental qualities.... Its combination with the scientific imagination and command of fact is still rarer and more precious." And in this common deficiency lies one of the reasons why the progress made in research is slow at the best, and why it may at the worst be nothing at all.

CHAPTER XIX

EXPERIMENTALISM

TRY it out and see is one of the first rules of successful research. Although the effective investigator has to do a certain amount of theorizing, he is not so much a dreamer as a doer. Centuries ago, long before experiment was generally practised or even permitted, Francis Bacon wrote the following parable, having as its moral the importance of direct observation:

In the year of our Lord 1432 there arose a grievous quarrel among the brethren over the number of teeth in the mouth of a horse. For thirteen days the disputation raged without ceasing. All the ancient books and chronicles were fetched out, and wonderful and ponderous erudition, such as was never before heard of in this region, was made manifest. At the beginning of the fourteenth day, a youthful friar of goodly bearing asked his learned superiors for permission to add a word, and straightway, to the wonderment of the disputants whose deep wisdom he sore vexed, he beseeched them to unband in a manner coarse and unheard-of and to look in the open mouth of a horse and find answer to their questionings.

It was a century and a half after the date mentioned by Bacon that Galileo, one of the founders of the experimental method, began to leave his lasting impress on the world. Galileo put to the test of experiment some

of the things believed to be the teachings of Aristotle. One of these—a question which had been debated for two thousand years, but, inconceivable though it may be to us, never properly tested—was whether a heavy body falls faster than a light one. Instead of joining the debating society, young Professor Galileo announced that he was going to try dropping two balls, one a hundred times as heavy as the other, from the gallery of the marble tower which leaned so conveniently there in Pisa, and see what nature said about it. One morning in 1591, before fellow members of the faculty of the University of Pisa and others, he performed his simple but famous and important experiment. And, to the great surprise of many of the observers, the heavy ball and the light one struck the ground at exactly the same moment.

A theory, no matter how logical it may be, is only an opinion. Until it has been definitely proven by persistent experimentation, theory must remain opinion. It is upon the indefiniteness of opinion unsupported by experiment that arguments are based. "Argument," in the words of a contemporary engineer, "is always the indication of a lack of knowledge, and when two fellows argue either one of them is right and the other is wrong, or they are both part right and part wrong, or both of them are wrong—and it is usually the latter."

In research the rule is simple enough. If one wants to know the length of a table or how tall a totem pole is, he does not guess or speculate, but he gets a yard-

stick and measures it. After that has been done there will no longer be anything to speculate or theorize about. But putting this simple rule into practice is sometimes not so easy. If the investigator happens to be out in a new and entirely unexplored field, as he very often is, then he may have to invent some new and different measuring sticks before he can scale the peculiar tables and the queer totem poles he meets there. He can not measure the diameter of a molecule or the length of a light wave with nothing but a yardstick. That is why one of the biggest problems in research is that of developing instruments of observation and measurement.

As has been pointed out already in the chapters on Accident and Imagination, experiments have a habit of yielding unexpected results. "Microbe hunters usually find other things than they set out to look for," said de Kruif. Many of these unexpected observations are worthless, of course. Sir Francis Darwin has told how his father, Charles Darwin, used to say sometimes as he left his greenhouse after some disappointing experiment, "The little beasts are doing just what I did not want them to do." But by no means all the unexpected results of experiment are worthless. Often quite the reverse. The efforts of the chemists, Caro and Franke, to find a new way of making cyanide for use in extracting gold yielded an unexpected but perhaps even more valuable compound, calcium cyanamid. Pasteur was injecting into chickens the microbes that

cause cholera when one day, the supply of fresh birds having been low, he happened to include two that had already had cholera and got over it. Pasteur expected that every one would die, as all the fresh chickens did. But, to his great surprise, the two that had already had cholera and recovered from it were not affected in the least by the virulent dose of cholera germs they had received. Thus was the observing Pasteur led unexpectedly to the principle of immunization.

When, during that historical Easter vacation of 1856—a date which Dr. Slosson said is as well worth remembering as 1066—the youthful William Perkin was trying to convert aniline into quinine, he made instead the first one of the aniline dyes, the purple mauve. Quinine neither he nor any one since has ever made from aniline. But, in Perkin's unsuccessful attempt to reduce the chills of the world, he did make it a more colorful place to live in. What if in his disappointment over the failure to get quinine he had failed to observe and seize upon the much more important product that he had made so unexpectedly!

Because the results of experimentation can not be predicted, the ability to contrive fool-proof experiments, and then to observe what actually does happen when they are made, is probably a more important qualification for an investigator to have than facility at constructing theories. "Theory is the general, experiments are the soldiers," said Leonardo da Vinci. "Experiment ...is never wrong; but our judgment is sometimes de-

ceived because we are expecting results which experiment refuses to give."

It is because of the great complexity of things in general, and of the imperfection of knowledge in every field, that it is not possible without experimentation to predict with precision the validity of any theory or the outcome of any experiment, however simple. And so, although it is important at times for a research worker to be what Wilder D. Bancroft calls a "guesser," as was mentioned in the essay on "Imagination," it is usually important that he have also some facility as an "accumulator," or in other words that he be an experimenter.

CHAPTER XX

ENTHUSIASM

THE "*Eureka! Eureka!*" of Archimedes as he jumped out of his bath and ran naked and shouting into the street is perhaps the earliest record and the best known one of the enthusiasm of the investigator. Archimedes had been trying to find a way to tell whether a certain crown supposed to be made altogether of gold really was pure gold, or whether it was alloyed with silver. One day as he stepped into his bath which was full to the top, so the story goes, he observed the displaced water running over, and it there occurred to him that the greater bulk which the crown would have if it contained any silver could be found if only he were to put equal weights of gold and silver separately into a vessel full of water and note the relative amounts of water that overflowed.

It was when this simple solution of the problem he had been struggling with suddenly came to Archimedes that he rushed out with his shout of "*Eureka! Eureka!*" There was just as much triumph in the shout of Archimedes as there was in the terse saying of Cæsar after the defeat of Pharnaces at Zela: "*Veni, vidi, vici*"—I came, I saw, I conquered. And it may well be that, so far as ultimate effect upon future events was

concerned, the enthusiasm of Archimedes had as much to justify it as did that of Cæsar.

At any rate, enthusiasm is one of the qualities most necessary to success in research, just as it is in any difficult, up-hill fight. "Nothing great was ever achieved without enthusiasm," said Emerson. And there is no question that that is true, so far as the job of research is concerned, for it is the enthusiasm which makes a worker keep persistently at his search, in spite of the inevitable difficulties and disappointments, that makes him win out.

That great searcher, Pasteur, whose enthusiasm often made him turn his nights into days, wrote one time, "I am on the verge of mysteries and the veil is getting thinner and thinner. The nights seem to me too long. I am often scolded by Madam Pasteur, but I tell her I shall lead her to fame." One of the records made in his note-book by Thomas A. Edison when he was working on the incandescent lamp reads thus: "Brought up lamp higher than a 16-c.p. 240 was ever brought before—hurrah!" That last word goes a long way toward explaining why it was that Edison often used to live at his laboratory day and night. Another statement credited to Edison, as having been made at the time he and his men were working on the motion picture machine just as the first encouraging results were secured, was this: "That's it; we've got it! Now work like hell."

Justus Liebig told that, whenever he and Gay-Lussac were successful in discovering an especially interesting

fact, they danced around the table together, "the boy of twenty and the older boy of forty-five." So intensely interesting was nature to Charles Darwin that, when he first began to study birds, he kept wondering why every man did not become an ornithologist. When in October, 1806, Humphry Davy discovered that, by means of electrolysis, metallic sodium could be made out of soda and potassium out of potash, he is said to have bounded about the room in ecstatic delight. But so great had been the enthusiastic concentration with which Davy had pursued the investigations which were then at last successful that his health broke and he was so sick for a period of several weeks that his life was despaired of. All London was said to have been agitated over the expected death of the young chemist, and bulletins on his condition were issued by his physicians morning, noon, and night. Who thinks that popular interest in science and in scientific workers is only recent?

Mrs. Einstein has said of her famous husband that "He works like an artist. He sees a vision...he works feverishly...his temperature rises, his face becomes flushed." And she added that, whenever Einstein is working in that way, she—wise wife—makes it a rule to leave him strictly alone.

Sometimes, although not often to be sure, a new discovery once made is so spectacular as to kindle enthusiasm even in those who simply see the results, without having had any part in achieving them. The

first airplane flight made at Kitty Hawk by Wilbur and Orville Wright was an instance of this. The story is told that one of the bystanders who saw that first flight rushed away in his excitement with the ungrammatical exclamation, "They done it! They done it! Danged if they ain't flew!"

The enthusiasm of the explorer is not at all surprising; for, said Francis Garvan, "great causes are just as intoxicating as night clubs." And it is characteristic of the research worker to believe that whatever he has chosen to work on is a thing of importance. It is that belief which in large measure is responsible for the enthusiasm of the investigator. James Clerk Maxwell wrote to one of his intimate friends in 1865: "I have a paper afloat, with an electromagnetic theory of light, which, till I am convinced to the contrary, I hold to be great guns." That, said Michael Pupin who told about it, was "a very strong claim made by the most modest of men! The paper was presented during that year to the Royal Society and was 'great guns.' It makes, like Newton's discovery of the law of gravitation and his formulation of the laws of dynamics, a new epoch in science."

Although a certain amount of enthusiasm is thus essential to success in research, it is important for the investigator not to let his enthusiasm lead him on to do foolish things. Some one has told a story about a dog who chased a rabbit into a hole, and, as dogs will, at once began feverishly to try to dig him out. But, after

the dog had dug and dug and dug, without finding any rabbit, he discovered that all Mr. Rabbit had done was to run in and out again at another hole—a thing which the dog could easily have found out much sooner, if only he had not been too intent on his enthusiastic digging to make a little general survey of the ground first. Unfortunately, research workers are sometimes just as blindly enthusiastic in their digging as that dog was. But enthusiasm is never unlimited in amount; and, if it is always to hold out until jobs are finished, it is essential that it should not be wasted, but be wisely and intelligently directed.

CHAPTER XXI

PATIENCE

SOME years ago when Charles F. Kettering was doing research for the National Cash Register Company, the head of the company, John H. Patterson, came to him one day to inquire how soon a certain development would be completed.

"In about a year," Mr. Kettering estimated.

"All right, then, I want you to double your force and finish it up in six months."

"I am sorry, Mr. Patterson, but that can not be done. It will take just about one year to finish the job."

"Do you mean to tell me, sir, that you could not do double the work on this project if you had twice as many men?"

"Maybe I can give you the best answer to that question, Mr. Patterson, by asking you another. Do you think that by putting two hens on the nest a setting of eggs could be hatched out in less time than three weeks?"

This matter of the considerable length of time required to get definite results in research is one of the things about it that is least commonly appreciated. It is not ordinarily understood by people in general nor by the executive in industry who must provide the

funds for the support of industrial research. And not always is it fully appreciated even by those who are engaged in research itself.

The late John E. Teeple coined the phrase "patient money" to signify the necessity of long-continued support of research, if worth-while results are to be obtained. And it is certain that great financial patience in the support of research is essential to any appreciable accomplishment. But of equal importance is patience in the research worker himself. It may take years to finish a research project, and in fact usually does. "The only progress which can be rapid is progress downward," said Sir James Jeans. There are plenty of places along the rocky, up-hill road to any desirable objective where, if not sufficiently fortified with patience, the investigator as well as those who support his efforts may succumb to discouragement. Of research may be repeated what Sir Esme Howard said about diplomacy: "The great art of diplomacy is patience, patience and again patience."

The Badische Anilin-und-Soda Fabrik spent fifteen years of patient research and five million dollars in "patient money" before they learned how to make synthetic indigo. W. D. Coolidge and his helpers put five years of patience into the research on how to make the tungsten filaments in electric lights ductile and less fragile. More than fifteen years of patient effort and many millions of dollars have already been expended in learning how to make gasoline out of coal, and the re-

search on that project is still being pursued in an intensive manner. Years and years of patient research had to be supported and done before rayon and similar synthetic textiles were converted from a dream into a reality.

It took Pasteur five years to find his remedy for hydrophobia. Paul Ehrlich spent seven patient years in making hundreds of hit and miss tests with discouraging results, before he finally arrived at his great discovery "606," which he named after the number of compounds tested during his long search. Charles Goodyear in his research on rubber struggled along through the darkness for ten years before the lucky accident that taught him how to vulcanize rubber. For ten years also Michael Faraday labored in his effort to "change magnetism into electricity" before he was successful. And in spite of his patient endeavor, Faraday did not always feel sure of ultimate success. In 1831 he wrote to a friend: "I think I have got hold of a good thing but can't say. It may be a weed instead of a fish that, after all my labor, I may pull up." It was certainly no weed that Faraday finally did pull up. John Tyndall said of it that it was "the greatest result ever obtained."

One reason why research takes a great deal of time is that it consists pretty largely of an educational process—of an education of the research worker himself—and education is always slow. If only an investigator could have the requisite knowledge in advance, there

would be no need for the research—and, incidentally, he might soon be out of a job. But the limitations of the human mind are such that knowledge comes to it very slowly at best, and as a result of much endeavor. This is particularly so in the case of a search for knowledge that is entirely new, which is chiefly what research consists of. In order to educate himself in a new field, or one that has not been explored before, a research worker often has to develop and build his own tools, such as instruments for measuring or indicating obscure phenomena; he must make multitudes of observations; and he must correlate and analyze masses of data, sometimes rejecting much that has been secured at the expenditure of a great deal of time and effort. Groping around in the dark as he is, he goes into many blind alleys and finally reaches his objective after traveling a very devious route. After having finished a piece of research, the same investigator could do it over again in much less time, of course. It is like the postscript of Madam de Sévigné's letter to her daughter: "If I had had more time I should have written you a shorter letter."

There are other reasons, too, why research moves slowly, such as failure to organize it properly, to use as good judgment as might be on the line of endeavor to follow, to pursue the investigation in a concentrated and uninterrupted manner, to keep from going off on useless tangents, and to eliminate the delays resulting from discouragement; but the one outstanding reason

why research takes a great deal of time is the slowness with which new knowledge can be acquired at best. And enlarging the staff does not always hasten that process.

CHAPTER XXII

PERSISTENCE

THE normal course of an endeavor in research is about as follows: First, an idea or a vision of some new thing or some improvement of an already existing thing comes to an investigator. Next, in the initial enthusiasm that usually follows such a conception, he sets out to convert the idea into a practical reality. It usually happens that, for a time, what appears to be rapid progress toward the desired objective is made, although, of course, the apparent progress may not begin until after a great deal of work has first been done. Then, when it begins to appear as though the reaching of the objective should be pretty well assured, there comes that inevitable time when a number of unforeseen barriers arise to block the pathway to success. Before the job can be finished, these barriers must be tackled one by one, and either be removed or got around. And usually it takes a long, hard, up-hill fight to do that.

It is during this tough fight that many a research project has been allowed to die. The reason is that some investigators do not have enough tenacity of purpose or enough persistence to stick to the battle until it is won. Gamaliel Bradford quoted Thomas A. Edison as having said this: "In working out an invention, the

most important quality is persistence. Nearly every man who develops a new idea works it up to a point where it looks impossible and then he gets discouraged. That's not the place to get discouraged, that's the place to get interested. Hard work and forever sticking to a thing until it's done are the main things an inventor needs."

The story of Stephen Babcock and Sylvia illustrates the fact that this necessity for persistent endeavor may continue on even after a development appears to have been successfully completed. Stephen Babcock, then a young instructor at the University of Wisconsin, was assigned the job of devising a much needed test for determining the amount of butterfat in milk. After a great deal of endeavor, he devised a test that fully satisfied his associates, and they urged him to give it out to the world. But Babcock had discovered that there was one cow in the University of Wisconsin herd, named Sylvia, whose milk was so rich that it would not check on butterfat content by his test. So Babcock disconnected his telephone, as was his custom whenever he began work on a difficult job, and went at it again, with the aid of Sylvia, in the effort to put the joke about the dairyman's pump completely into the discard. He was at last successful in doing that, and in doing it so well that the Babcock test of to-day is substantially the same as that which he first gave to the dairy industry of Wisconsin and the world.

Being a good starter is one of the most common of

human qualities, but the much more important quality of being a good finisher is rare indeed. This deficiency is not confined to research, of course; but there is particular danger of the efforts of a research worker being like Samuel Taylor Coleridge's works on metaphysics. Charles Lamb said of Coleridge that he started many treatises on metaphysics but never finished any of them.

Dr. Frank Crane once wrote a little essay on the theme "keep on keeping on," and one of the things he said was that if a bicycle ever stops it falls over. So whenever a research worker fails in an experiment, or even in a major endeavor, he should take the "army cure," lest he lose his nerve or his enthusiasm, or both. (In the army air service it is said to be the rule that a flier takes the air again as soon as possible after an accident, to serve as a quick nerve-restorative.) "Give me the fellow who will stick to the job until he finds an answer," said President W. C. Teagle of the Standard Oil Company of New Jersey. "The other corporations can have the geniuses."

In his note-book, Thomas A. Edison would sometimes place opposite the record of an unsuccessful experiment the letters "T.A." "T.A." did not stand for Edison's name. Instead, it meant "Try Again." And "Try it again" is one of the first rules of successful experimentation. The character of research is such that the principles of Robert's *Rules of Order* can not be made the guide for doing it successfully, for in parliamentary procedure there are several motions that can not be

amended. One of these is an amendment of an amendment. But in research amendments of amendments must be amended, and amended yet again, often many, many times. The persistence of a bulldog is what the research worker needs. "It's dogged as does it," said Charles Darwin.

It is the unlooked-for obstacles which are met with in the effort to develop any new idea that makes persistence a more valuable quality to the research worker than brilliance. The ideal situation would, of course, be for every investigator to have a good measure of both of these qualities; but, of the two, persistence is the more important, just as it was in Æsop's fable of the tortoise and the hare. Persistence is perhaps more a quality of maturity than of youth. So, although the qualities that go along with youth are distinctly valuable in research, they need, for best results, to be supplemented with some others as well. And not the least of these is persistence.

One thing that has been played up in connection with the up-to-date explorer is the day-and-night hours that some of them have kept. There is no question that enthusiasm and the fascination of the job have made many an investigator work day and night for certain periods. But, after all, the thing that counts most is the consistent and persistent, day-after-day struggle. As a matter of fact, a research worker may get more done in the end if he concentrates on the job during the regular hours of the day when his mind and his

enthusiasm are fresh, and then rests at night. "Regular hours of work are really better than irregular hours in the long run," says research director C. E. K. Mees of the Eastman Kodak Company.

But, however he does his work, the characteristic difficulties that are always met with in research make the advice of William Edward Hickson's familiar poem particularly applicable to the investigator:

'Tis a lesson you should heed,
Try again;
If at first you don't succeed,
Try again;
Then your courage should appear
For if you will *persevere*,
You will conquer, never fear,
Try again.

As applied to research, Charles F. Kettering has made a more concise, if less poetic, expression of the same idea: "The only time you don't want to fail is the last time you try."

CHAPTER XXIII

FAITH

YOU can't make a silk purse out of sow's ear," is an old saying that used to be considered as expressing the ultimate degree of impossibility. But to-day that saying has no force at all. The reason is that an outstanding American chemist, Arthur D. Little, who had more faith in chemistry than to believe that such a thing was impossible, did make a silk purse out of a sow's ear. That purse, with its pretty red and blue tassels, has ever since been on exhibition at Dr. Little's laboratory in Cambridge, Massachusetts. It is a silent witness of what faith, coupled with intelligent action, can sometimes do toward converting impossibles into realities.

To see around a corner used to be thought of as altogether impossible. But some one who did not know such a thing could not be done invented the periscope, so he could look around not just one corner, but two—or even three. Nothing used to be more impossible than that a man should lift himself by his bootstraps—until some one found out how to multiply effort by means of the familiar arrangement of pulleys and cords called a block and tackle. Since that time a man could not only lift himself, but also the platform on which

he stood, together with all it was laden with, just as painters and other artisans working on buildings constantly do as a regular thing. Finding a needle in a haystack, another one of the popular impossibles, would not really be difficult for one provided with a good strong magnet of large area.

Edgar A. Guest said of Thomas A. Edison that "Impossible was king when he was born." And certain it is that Edison converted some *im*-possibles of his day into possibles. One of the qualities that plays a large part in such accomplishments is faith. And Edison was gifted with a large measure of that quality. Once Edison came down to his laboratory in East Orange to find it in ashes. As he stood there and looked at the smoking embers of the institution in which his life's work was centered, his only comment was this: "To-morrow we shall have the joy of beginning all over again." It was that quenchless faith of his in ultimate success, which made Edison begin all over again upon the failure of any endeavor, that made it possible for him so often to do the impossible.

Anything that a fellow does not know how to do is impossible of course—impossible for him, at least. And if he believes that a thing really is impossible, simply because he does not then know how to do it, he is deficient in one of the most essential qualities of a successful research worker—or, for that matter, of any other kind of pioneer.

The faith of the research worker needs sometimes

to be greater than merely that required to overcome the customary obstacles met with in the search for new information. At times it needs to be great enough to survive the withering effects of what Wilder D. Bancroft has called the misleading experiment. "In December, 1824," said Dr. Bancroft, "Faraday attempted to obtain an electric current by means of a magnet, and on three occasions he had made elaborate but unsuccessful attempts to produce a current in one wire by means of a current in another wire or by a magnet. It was not until August, 1831, nearly seven years later, that he succeeded in making the experiment fit the theory. He had even more trouble with the electro-magnetic rotation of the plane of polarized light, for it took him more than twenty years to get a positive result. People say that this long series of researches is an instance of his perseverance; but there would have been no sense in persevering if he had been wrong. In that case he would have been called pig-headed. The important thing was that he knew all the time that the negative results were misleading and that the theory was better than the experiments."

Dr. Bancroft cites also the case of Emile Roux, whose firm conviction finally triumphed over recalcitrant experiments in his search for the cause of diphtheria. As the story was told by Paul de Kruif in *Microbe Hunters*, it seemed to Roux that the bacillus observed by Loeffler was the cause of diphtheria. But he could not find any traces of the bacilli in the tissues of rabbits dead from

injections of broth in which the microbes had been grown. Then he recalled Loeffler's speculation that it must be that the germs generate a poison in the broth; and it was that poison, not the bacilli themselves, which paralyzed and killed the rabbits. So to test out this idea Roux "took big glass bottles and put microbeless soup into them, and sowed pure cultivations of the diphtheria bacillus in this broth; into the incubating ovens went the large-bellied bottles." At the end of four days they filtered the soup. The filtrate was injected into the rabbits and guinea-pigs with no result. The animals did not die.

But, in spite of this strong evidence to the contrary, Roux persisted. He next injected such a large amount of the filtered soup into guinea-pigs and rabbits that "it was as if he had put a bucketful of it into the veins of a middle-sized man." Still there was apparently no effect, not even from that "ocean of filtered juice" itself. Two days later, though, the animals did get sick, and "in five days they were dead, with exactly those symptoms their brothers had, after injections of the living diphtheria bacilli. So it was that Emile Roux discovered the diphtheria poison." But he would not have done it if he had not had too much faith in his idea—in the idea that the diphtheria microbes in a child's throat generate enough toxin to kill the child—to believe the results of his own experiment.

A striking instance of the lack of faith in a new thing—or perhaps of some deficiency of imagination as

well—was given by Thomas A. Watson in his account of "The Birth and Babyhood of the Telephone." This was the same Watson who helped Alexander Graham Bell in his experiments and who was present at the birth of the telephone. Telling of events back in 1881, Watson said: "As the telephone business had become, I thought, merely a matter of routine, with nothing more to do except pay dividends and fight infringers, I resigned my position as General Inspector of the Company, and went over the ocean for the first time." In the light of progress since then, and of the fact that even to-day, more than fifty years after the time spoken of by Watson, the leaders of the telephone business think it wise to maintain the largest research organization in the country, the Bell Telephone Laboratories, Watson's decision seems now not to have had back of it as much faith in the future of the telephone as was warranted.

There is one kind of research in which a useful result or a usable product is secured fairly early in the course of the endeavor, which result can be used or marketed immediately, and then improved further by continual research. There is another kind of research in which years and years of patient endeavor are required to reach any definite objective. The first requires less in the way of sustaining faith than the second, although the ultimate amount of research that is done on the project may be the same in both cases. The telephone is an instance of the first form of research spoken

of above. An instance of the second form is the fifteen-year research of Adolf von Baeyer and his associates, which resulted finally in the discovery of how to make indigo by synthetic means.

But whatever the character of a piece of pioneer work may be, faith in the ultimate outcome of the necessary exploration is one of the most essential qualities for those who undertake it to have. As W. M. Grosvenor has suggested, it has always been the man who did not believe that certain things could not be done who "revolutionized affairs and opened up new horizons of progress.... He had his own vision, made his own sacrifices to his own gods, and there was no one to say him nay; so he kept on trying till it happened."

CHAPTER XXIV

COURAGE

THE spirit of high adventure still lives," said Herbert Hoover to Dr. Hugo Eckner after his experimental flight around the world in the Graf Zeppelin. And one of the places in which it lives is in the field of research.

It would perhaps be easy to match every recorded instance of great courage or heroism in conventional exploration with just as great a one among the experimenters and research workers along various lines, who are the explorers up-to-date. Thus, during the 1865 epidemic of cholera in Paris, Pasteur established himself right over the cholera ward of a hospital in order that he might study the disease first-hand. Fearing for Pasteur's safety, Henri Saint Claire Deville said to him, "Studies of that sort require much courage." To this Pasteur replied, "What about duty?" When the Asiatic cholera broke out in Alexandria some years later, Pasteur was deep in his research on rabies. But he sent to Alexandria his helpers, Emile Roux and L. Thuillier. Soon the search of these two for the bacillus of cholera was rudely interrupted, for Thuillier contracted the disease and died. Robert Koch, the great German experimenter who was studying cholera there also, and who

later succeeded in finding the "comma" microbe of cholera, said of the wreaths he laid upon martyr Thuillier's coffin, "They are very simple, but they are of laurel such as are given to the brave."

Dr. Hideyo Noguchi of the Rockefeller Institute went to the Gold Coast of Africa to prosecute his research on yellow fever. He did so right in the face of the fact that he had been in uncertain health himself and that he was well aware of the danger of going there, where he later died of the dread disease he was investigating. Equally great was the heroism of those other warriors against yellow fever who helped Walter Reed find out about the disease by being the human guinea pigs for his experiments: James Carroll, William Dean, Private Kissinger, John J. Moran, W. G. Jernegan, L. E. Folk, and the young Dr. Cooke, together with those other but nameless men who volunteered to expose themselves in various ways to the justly-feared disease.

But sometimes the greatest courage is exhibited in cases where the spice of danger is not so apparent. One day, during the period when Charles F. Kettering, almost single-handed, was developing the self-starter for the automobile, his experimental car slid off the road breaking his leg. And the very next day the garage at the Cadillac Motor Car Company that housed the car on which was the only other self-starter in existence, burned to the ground. If all the progress that had been made up to that time toward getting a successful self-starter were not to be lost, somebody would have to put

one of them into commission again, in order that the performance tests at Cadillac might be continued. And, because the only man among his helpers who he thought could do that failed in the attempt, Mr. Kettering himself then got up out of bed, although it was only two days after his leg had been broken. He rode two hundred miles on a train, got beneath the damaged car, found out what the trouble was, and put the precious self-starter into operation again. This simple heroism of his is one of the reasons why people do not still have to crank automobiles by hand.

The courage here shown by Mr. Kettering was of two kinds: first, the heroism of doing what he did in spite of all the anguish it caused him; and, second, the courage of believing so firmly in a new thing—in a thing that every one told him could not possibly succeed. The latter kind of courage was perhaps the greater of the two. If not the greater, it was at least the more difficult. It is a high form of courage that is required to face and to overcome the humdrum discouragements which accompany research. And there are times when this high courage of faith is essential to the success of every endeavor in research; for, just as in other worth-while human endeavors, the research worker has to spend the most of his time in hard, tedious, unromantic toil.

In the British Museum, the Louvre, and other European museums are precious collections of Palissy's pottery. Back of those exhibits is an unusual instance of

this rare courage that refuses to succumb to persistent failure in experiment. Bernard Palissy, who lived during the sixteenth century, set himself to make a superior kind of pottery. For sixteen years he struggled along with his persistent experiments through a succession of failures. All that time he was, as he said, "like a man who gropes in the dark." At times he and his family were reduced to poverty. To feed his furnaces, he finally burned his furniture, and, it has been said, even the very floor boards of his house. The now carefully preserved exhibits of Palissy's pottery show how well this courageous experimenter ultimately succeeded.

It is but natural that there should be just as much of the noble qualities of courage and heroism in research as there ever was in exploration. Forgetfulness of self in the enthusiasm of the push toward a desired objective is both a characteristic of the true research worker and the essential background of heroism. And the quality of self-forgetfulness is certainly as common, and probably much more common, among research workers as it ever was among conventional explorers. True heroism is perhaps not a necessary quality for the ordinary research worker to have, but the really effective investigator must have a reasonable measure of courage, moral as well as physical. And it is gratifying to know that the still more popular, if not more noble, quality of heroism itself does find a place among up-to-date explorers.

CHAPTER XXV

COMMON SENSE

THERE is perhaps no field of endeavor in which common sense is more conspicuously important than in research. "Science is, I believe, nothing but trained and organized common sense," said Thomas Huxley. Charles F. Kettering also has said that research must partake as much of economic "horse" sense as it does of scientific principles.

When Theobald Smith set out to find the cause of the Texas fever which was killing off great numbers of American cattle in a mysterious manner, he was told, as related by Paul de Kruif in *Microbe Hunters*, that wise old western cattle growers had a notion that Texas fever was caused by an insect which lived on the cattle and sucked their blood, and which the cattle men called a *tick*. Although all the learned doctors of the various state Experiment Stations scorned such an idea, Theobald Smith had common sense enough to listen to it and then to subject it to careful test. The result was that he "proved those western cowmen to have observed a great new fact of nature....He chiseled that fact out of folk-shrewdness." By his common-sense appreciation of the possible validity of the practical cowmen's hunch, and by his subsequent painstaking

ing and practical experiments in tracing the history of the insects and of the microbes they carried, he not only showed how to control and to prevent Texas fever, but also he established the conspicuously important fact that a disease may be carried by an insect. And the latter of Theobald Smith's results was of the highest importance in directing the searches of later microbe hunters.

It certainly is true that in research, just as in other fields of endeavor, there is a large place for that form of intuitive judgment which is based upon something that it is not always possible to define exactly. It is a little like what Edwin E. Slosson said about the reasons why an editor accepts or rejects a manuscript submitted to him. Dr. Slosson said that no one should ever ask an editor for the reasons why he rejected a manuscript; for, although the editor is perhaps right in his judgment, it is likely that he could not always tell just what it was based on.

The quality of common sense has always been one of the important factors in the search for new knowledge. With the aid of it, the ancient Mayans of Yucatan worked out a calendar which was accurate to the loss of only one day in 374,000 years. Their astronomers had no telescopes, but they must have had a full measure of that quality which is the subject of this essay. For their solar observations they used nothing but the circular wall of a tower in which long slits opening in many different directions were cut. One of those slits

bisects the sun on the longest day of the year. (It is possible to speak here in the present tense, for one of the towers with its observation slits is still there.) Another bisects the sun on the shortest day of the year, and still others have equally definite functions. So it was that with no instruments of precision, no equipment other than some holes in a wall, the Mayan astronomers succeeded in developing the most accurate calendar ever devised.

When Isaac Newton was sixteen he wanted to know what was the force of a gale that was blowing one day. He had no accurate instruments for measuring it; but, instead of sitting down and wishing that he had, he proceeded to get a value by the following simple means. He went out in the storm and jumped first with the wind, and second against the wind. By measuring the length of the leap in each direction and by comparing the results with the distance he could jump on a calm day, he arrived at an approximate figure for the force of the storm.

One of the principal points of application of common sense in research is in connection with the large part that is often played in research by human psychology. In an address which he gave before the Franklin Institute, Willis R. Whitney told a story of one of his own researches that illustrates the great importance of the research worker being at times, as some one has expressed it, "a practicing psychologist of sound common sense." There was a need in the electrical industry for

million-ohm porcelain resistances. Dr. Whitney's men, who set out to help the porcelain makers provide such resistances, recognized that ceramics is one of the oldest industries and they "learned that arts and industries contain much knowledge not recognized within the confines of any one science." So the searchers for high resistances first learned about ceramics from the practical workers in that field. Then, by careful experimentation in the control of ingredients and of firing technique, by the application of knowledge which they had from other fields, and by the aid of the practical workers in porcelain, they succeeded in doing even more in the way of improving the art of making resistors than they had set out to do.

Besides, and perhaps equally important, they accomplished another very desirable thing, which in the words of Dr. Whitney was as follows: "In learning from those who knew porcelain we were wise. By thus learning how we could help them, we turned the natural scorn of good, practical men for meddlers, into a feeling of satisfaction that there were added to their department a new product and interesting new electrical furnaces. And that department delighted in helping us ever after." It is this fostering of understanding contacts with those to whom the research laboratory can be of assistance—and who can be of assistance to the research laboratory as well—which is at once one of the most important and the most difficult tasks of the industrial research worker.

Charles F. Kettering tells that, upon receiving one day a sample of a new kind of steel, he gave it to a man in the shop with instructions to drill out some borings for the chemist to analyze. A little later he came back that way and asked the machine man whether he had done as requested. "No," he replied, "I couldn't drill that steel at all. It was so hard it turned the point of the drill right over."

"Did you try a diamond-pointed drill on it?" asked Mr. Kettering.

No, he had not done that. When he did try the diamond-pointed drill, he got the steel borings with no trouble. So in reality it was not that the steel was too hard, as the mechanic thought, but that the drill was too soft. That was where the difficulty lay; and it is the same point at which the difficulty lies in some unsuccessful researches.

Besides the value that common sense has in helping a research worker decide *how* to conduct investigations, it can be a great aid to him also in another important respect. And that is in helping him decide *what* investigations are likely to be worth while, and what are not. So far as such decisions apply to research in industrial chemistry—which, however, is pretty representative of industrial research in general—Charles M. A. Stine has said this about them: "It is as much the business of the research chemist to differentiate between what is feasible and what is not feasible, without wasting a lot of his employer's money on bad ideas, as it

is his function to bring into successful production the sound products of the research laboratory."

It is perhaps because research is as much, or more, of an art than a science that "good, sound, ordinary sense" is so essential to those who work at it. At any rate, as Wilder D. Bancroft has put it, "One must use common sense even when doing research."

CHAPTER XXVI

HONESTY

HONESTY is said to be the best policy. In research it is distinctly that, and more. There honesty has to be not only a policy but also a universal creed. Nowhere else is insincerity so much out of place as in a searcher after truth.

Honesty is the one single attribute that Kipling names in connection with the sextet of inquisitors enumerated in one of his poems:

I keep six honest serving men
(They taught me all I knew);
Their names are What and Why and When
and How and Where and Who.

Those four lines contain what is really a good characterization of the effective investigator—an honest inquisitor of nature.

It is most essential that any one who attempts to do research be intellectually honest in the strictest sense of the word. He must, first of all, be honest with himself. He must face facts, and be absolutely loyal to them. He must be free enough from prejudice and pre-formed ideas to see things as they really are—not as he might like for them to be. To set out on an investigation with the express purpose of substantiating some pre-formed

conception, or of trying to find evidence in favor of some "pet" idea, is a hazardous thing for any one to do; but, unfortunately, it is a thing which is sometimes done in certain circles, and done under the guise of conducting research.

The magazine *Judge* may not be thought of as a good source of advice on research. But nevertheless the following quotation is from that source: "Much of what passes by the name of research, in advertising for instance, is spurious. Starting with a conclusion and piling up data with a pretense of supporting it, is not research.... The safest procedure is to pay no heed to the man who tells you in advance what he expects to prove, but to sit at the feet of him, whose guiding principle is 'First let's get the facts.'"

In research there is no place whatever for that all too universal human habit of reaching decisions on the basis of feeling, rather than of fact. The research worker must know, as Dr. Slosson said, "how to steer his course by the fixed stars of fact, and not be misguided by the meteors of fancy." In common parlance, he must make sure not to "kid" himself.

It is important that the research worker be honest with others as well as with himself, of course. He must be honest with those who employ him, or those for whom he works directly; and he must be honest with the public, or those for whom the enterprise which furnishes his support really exists. But, if he is strictly honest with himself, honesty with every one else will

be the natural sequence, of course. Polonius, in the tragedy of *Hamlet*, might appropriately have been speaking to a research worker, instead of to his son Laertes, when he said,

This above all: to thine own self be true,
And it must follow, as the night the day,
Thou canst not then be false to any man.

It is fortunate that the training which must precede scientific endeavor, as well as the conventions which surround scientific work, are such as to foster intellectual integrity among those who are engaged in it. When Thomas Burr Osborne was a young man preparing for his subsequent career of distinguished service in science, he said to a group of young people with whom he was in conversation, "Training in a chemical laboratory does more to develop sound ethics than Sunday school lessons can ever do." That saying is equally true of the laboratories in other branches of science, and of those in engineering and industry as well. In all of them, intellectual dishonesty is equally certain to be discovered, and the penalty for it to be enforced. And no one wants to be dubbed a Dr. Cook.

Scientific men "have developed an elaborate method for detecting and discounting their prejudices," wrote Walter Lippmann in *A Preface to Morals*. "It consists of instruments of precision, an accurate vocabulary, controlled experiment, and the submission not only of their results but of their processes to the judgment of their peers." No discovery of importance is

ever reported by a scientific worker that is not effectively scrutinized by other workers in the form of attempts to repeat the original experiments. If, when this is done, the same results as were originally reported are to be obtained, it demands that the experiments of the pioneer shall have been accurately controlled, carefully observed, and faithfully reported. And, entirely aside from the usual innate honesty of the scientific man himself, his regard for his professional reputation demands that facts and not prejudices constitute the reports that he makes of his work. Every scientific worker is on the alert—and he should be on the alert—to keep the outstanding journals in his own field of science from becoming fiction magazines. So it is that publication has always been one of the important safeguards of truth in science.

There is thus a distinct difference between the ethics or the practices of the research worker and those, say, of the criminal lawyer. The researcher must base his representations or reports upon all the evidence that there is, both *pro* and *con*. But the lawyer defending a suspected criminal tries every means to make his client appear innocent of the charges preferred against him, even though he himself may know for sure that the man is guilty of them.

Michael Faraday gave the following as a definition of the correct attitude of a research worker: "The scientist should be a man willing to listen to every suggestion, but determined to judge for himself. He should

not be biased by appearances; have no favorite hypothesis; be of no school; and in doctrine have no master. He should not be a respecter of persons, but of things. Truth should be his primary object. If to these qualities be added industry, he may indeed hope to walk within the veil of the temple of nature."

CHAPTER XXVII

MODESTY

THE late Senator Dwight W. Morrow had a rule of personal conduct that he liked to call Rule No. 6. Rule No. 6 was this: "Don't take yourself too seriously." Along the same line, Arthur Brisbane said in one of his editorials: "Taking your work seriously is important. Taking yourself seriously is silly. A good hen lays the eggs and lets the rooster crow."

Though in research it does pay to take one's work seriously, there is a sense in which it does not do to take it too seriously. This sense is that the results which come from one's work should not be thought of as being greater nor more important than they really are.

Charles F. Kettering tells the story of how he sat one moonlit evening on the open porch of his Dayton home, Ridgleigh Terrace, with one of the country's outstanding radio engineers. The famous engineer was commenting upon what an amazing thing is modern radio, which enables people to sit at home and listen to music or speaking originating hundreds or thousands of miles away. Mr. Kettering's response was first to agree with what had been said, but second to suggest that people have always had a much more wonderful radio system than that spoken of by the radio

engineer. "That moon up there, 240,000 miles away," said Mr. Kettering, "is radioing to us over the system that I mean. Objects that are even millions of light years away signal to us nightly. The signals from them are received by a mechanism which is incomparably superior to the contraption of wires, tubes, rheostats, and condensers that we call a radio set. The radio receiver that I mean—the human eye—does not need to be closely tuned. It is sensitive all at once to the whole range of frequencies of vibration or wave lengths that we recognize as the various colors. And yet we take sight as a mere matter of course, while we think radio is wonderful. Radio *is* wonderful, but its wonder should not be over-rated." And that goes also for every development made through research.

"The very success of science puffs us up beyond all reason," said G. W. Stewart in an address as national president of Sigma Xi. . . . "We can at times hardly restrain ourselves from giving public utterance to speculations based upon experiments, that, when read by the public, are destructive of certain human values." In these days of the popularization of science, there is here, in the matter of failure to distinguish between speculation and the result of experiment, a special danger. Even the results of experiment, said Professor Stewart, "are frequently given too much confidence. The public prefers to assume that there is such a thing as absolute truth, and by inference, that science can and does ascertain fragments of it. But this is just what is not

true. All of our discovered 'laws' are not truths but only approximations." If that is so of the actual findings of investigators, what shall be said of their mere speculations, which so often are given out to people—to people in general, who do not know how to distinguish properly between speculation and the results of experiment?

There is a wide gulf between different investigators in respect to the degree of modesty—or of the lack of it—with which they view the results of the work they have done. When in 1930 the appointed committee were seeking nominations for the first of the annual awards of ten thousand dollars given by the *Popular Science Monthly* for "the achievement in science of greatest value to the public," at least three men nominated themselves. Needless to say, the work of none of these men appeared to the committee to be worthy of the award. Contrast with the conceit of these investigators what Alfred H. White said about the prominent organic chemist, Moses Gomberg: "And none of them (Professor Gomberg's students) will ever forget the interesting and logical presentation by the modest lecturer who never by any possibility referred to his own work more directly than to intimate that some of the research bearing on a particular subject had been performed 'in this laboratory.'" The great Sir Isaac Newton, in speaking shortly before his death of his estimate of his own achievements in science said: "I seem to have been only like a boy playing on the seashore, and divert-

ing myself in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me." Those are the modest words of one of the most prolific scientific workers who ever lived, discoverer, among many important things, of the differential and integral calculus, of the fundamental properties of light and color, of the laws of motion, and of the universal law of gravitation.

Those investigators who have been fortunate enough to make discoveries that are outstanding in importance should, in their estimates of their own work, not lose sight of how large a part mere luck, as distinguished from outstanding ability, sometimes plays in the making of such discoveries. "It is, indeed, a matter of doubt," said C. E. K. Mees, "how many of the men commonly considered to be of great genius by virtue of some important discovery they have made, really possessed any distinguishing ability compared with their fellows who did not have the fortune to make a similarly important discovery." On the occasion of his receiving from the German Emperor the Order of the Crown, with Star, for his achievements in finding the microbe of cholera and in showing how to control it, Robert Koch said: "If my success has been greater than that of most...the reason is that I came in my wandering through the medical field upon regions where the gold was still lying by the wayside...and that is no great merit."

Aside from the bad taste of too much conceit, and, in the case of the scientific explorer, of its possible bad effects upon people in general, there is another sense in which it is important that a modest view be taken of the results of discovery. Seeing the faults of a new thing—which, as experience has shown, every one of them has in abundance—is a very essential preliminary to the endeavor to correct the faults. This is the same principle as that which says that the first requisite for a fellow to begin to learn is to know that he does not know. Investigators are sometimes like the fond mama who dotes upon her spoiled and imperfect child, while she is altogether blind to the faults that others too easily see in it. The faults of such a child are never corrected, of course. And neither are those of new developments which are too highly appreciated by their sponsors.

PART V

ACHIEVEMENT

THE motive of geographic explorers was simply to find out about what existed in nature—and perhaps also to seize such natural wealth as the countries discovered happened to have. But in research the practical objective is not alone to find out about nature. It is also to go still further and improve upon nature. "If man cannot improve upon nature," said Dr. Slosson, "he has no motive for making anything." It is in bettering natural conditions of one kind or another that research has paid us its biggest dividends; although, of course, we have got other valuable things from it as well. Part V is concerned with what some of the specific achievements of research workers have been, and can be.

CHAPTER XXVIII

PRODUCTS IMPROVED

THE man who says, as some do, that he has “perfected” this device or that is talking the language of the egotist, or else of the ignorant. There is perhaps nothing at all that is altogether perfect. The ideals of the religionist are such that he believes that no matter how much a man may try to conform in faith and in life to the precepts of his religion he must still fall short of perfection. And much the same thing is true in any field of human endeavor.

L. A. Hawkins of the General Electric Company has told how “It was said in 1900 by one in a position to speak authoritatively that the incandescent lamp was then so perfect a device that it could never be much improved.” How foolish to make such a statement; and to do so in the face of the fact that in terms of the ideal light the incandescent lamp of that time had an efficiency only of about one per cent! That year was the one in which Willis R. Whitney started the General Electric Research Laboratory. Partly as a result of the immense amount of research carried on in that laboratory and partly as a result of research done elsewhere, incandescent lamps are now made which are more than five times as efficient as were those of

1900, when the authority—whose “authority” was evidently not valid—had said that no further improvement could be expected. But even to-day, having, as it does, an efficiency of less than 10 per cent as a producer of light, the electric lamp is a long way off from *perfection*. “Research, therefore, for more perfect conversion devices,” says Vice-President E. W. Rice, Jr., of General Electric, “is justified, and will go on.”

Now, to the present discussion, it is important to observe that the great improvement in the incandescent electric lamp was made possible not so much by improvements in the lamp as it existed in 1900 as by the incorporation into it of an altogether new discovery; or indeed of several discoveries, the most important of which perhaps was the value of metallic tungsten as a filament-fabricating material. That discovery was made by two Austrian experimenters, Just and Hanaman. So far as carbon-filament lamps were concerned, the “authority” was right, or at least no very large improvement was made in them. But an advance along an altogether different route, that was the way the incandescent lamp was improved, and it is the way many of the outstanding improvements in products are made.

The time may come in the life of any product, as it did in the case of the incandescent lamp, when further progress along the lines followed in the past appears doubtful. But that does not mean that perfection has

been reached. It is much more likely to mean that some altogether different kind of development is needed to open up another avenue along which progress can be made.

It must have been forgetfulness of this important principle of the existence of unexpected avenues of advance which made Henry L. Ellsworth, then U. S. Commissioner of Patents, say in 1844, "The advancements of the arts, from year to year, taxes our credulity and seems to presage the arrival of that period when human improvement must end." That was said before the coming of electricity, the airplane, the motor car, the telephone, the radio, the moving picture, and all the host of things developed since that time. At the very time Commissioner Ellsworth spoke, Elias Howe was working on his sewing machine, and Charles Goodyear had just made his great discovery of how to vulcanize rubber.

The same point about the large fields which are sometimes opened up by developments along altogether new lines is illustrated by a story related by R. A. Millikan in some one of his writings. Dr. Millikan told how in 1894 he attended a scientific lecture in which the speaker outlined a point of view held then by most scientific men. This was to the effect that all physical phenomena must of necessity fit into the complete, well-verified, and apparently all-inclusive set of laws and principles developed by scientific study up to that time. The speaker concluded by saying that it

was probable that all the great discoveries in physics had already been made.

"Just a little more than one year later," says Dr. Millikan, "and before I had ceased pondering over the aforementioned lecture, I was present in Berlin on Christmas Eve, 1895, when Professor Roentgen presented to the German Physical Society his first X-ray photographs.... As I listened and as the world listened, we all began to see that the nineteenth-century physicists had taken themselves too seriously, that we had not come quite as near sounding the depths of the universe, even in the matter of fundamental physical principles, as we thought we had."

The moral of all this is plain enough, of course. But, nevertheless, the importance of looking for altogether new lines along which to improve products is sometimes lost sight of, even by the men engaged in research.

Apparatus for producing power by means of steam is one thing that has been greatly improved through the efforts of the many experimenters in that field. Savery's engine, built around 1600, is said to have consumed about one hundred pounds of coal per horsepower-hour of output. The Watt steam engine of the last quarter of the eighteenth century consumed ten pounds of coal in doing one horsepower-hour of work. During the subsequent century, boilers and engines were slowly improved, until in 1900 a good power station could do the same work with half the coal, or

five pounds. By 1913, with the coming of the steam turbine, the amount of coal required per horsepower-hour of power plant output had been divided by two again, or decreased to less than two and one-half pounds. The continuation of bearish effort on coal consumption by persistent experimenters since that time has pushed the amount required per horsepower-hour, under favorable conditions, down to one pound or less. Thus it is that experiment has made the history of coal consumption per horsepower-hour of power plant output about as follows:

1600	100	pounds
1800	10	pounds
1900	5	pounds
1913	2.5	pounds
1930	1	pound, or less

From these figures it may seem that perfection has at last been reached. But not so, for the lowest figure given still represents an over-all thermal efficiency on the basis of the coal consumed of less than 25 per cent.

William McFee has told that during the time when ocean-going steamships were powered by the simple steam engine of pioneer days the entire design of a steamship had to be assembled about her coal-carrying capacity. So enormous was the amount of coal required that engineers agreed on the folly of imagining that any ship could carry enough "coals" to get her across the Atlantic. But later on, as a result of the continued experimentation already spoken of, came the multiple-

expansion condensing steam engine, which was applied to ships and which, by virtue of its higher efficiency, changed the situation altogether. But "If the compounding of steam engines had not been discovered," said Mr. McFee, "the sailing ship would have been the sovereign of the seas until the twentieth century.... The history of the world, as well as that of navigation, would have taken another route."

An important and common form of advance is that which is made possible, by some indirect development, perhaps by one that may appear to be small and totally unrelated to the usefulness of the product in question. Thus the most important contribution of alloy steels to the motor car is perhaps not their usefulness as materials of fabrication at all, but as the high speed cutting tools which enable the thousands of parts that each car consists of to be fashioned so rapidly, so cheaply, and at the same time with such high precision. Who would have thought that the neon lamp and the photo-electric cell would have had anything to do with seeing across the continent through a wire or through the wireless ether? But those were just the two products of research that television had to wait for. So also, the microphone which is so essential to radio broadcasting and to the making of talking pictures was not devised for those purposes. It was developed in the Bell Telephone Laboratories simply as a sensitive laboratory instrument for studying sound.

Perhaps every modern product has some such in-

direct but vital development incorporated in it. Such things are industrial illustrations of the old folk idea that "great events from small occasions rise." It is the same idea which forms the basis of the old jingle:

For want of a nail, the shoe was lost;
For want of the shoe, the horse was lost;
For want of the horse, the rider was lost;
For want of the rider, the battle was lost;
For want of the battle, the kingdom was lost;
And all for the want of a horseshoe nail!

There are industries which are still waiting for the kind of developments spoken of immediately above, of course. Thus, one thing that is greatly needed if the airplane is ever to reach the full measure of usefulness which it might have, is some means of seeing or feeling a reasonable distance through fog.

In searching for improvements, there is room for the exercise of some judgment as to what line to follow or in what way the desired result can best be obtained. In 1906, when to keep automobiles going it was necessary to crawl under them quite frequently, some one tried to reduce the inconvenience involved by devising an automobile body which would be tilted back out of the way when repairs to the car became necessary. But, since that time, the difficulty of crawling-under has been overcome in a much better manner, namely, by building cars in such a way that they hardly ever need the kind of attention which used to make it necessary to get beneath them. It is always

possible to roast a pig by burning down the house he lives in, as the Chinaman who first discovered the value of roasting pig is said to have done; but there is usually a better way of doing it.

For a long time it was known that certain gelatins were good for making photographic emulsions and that others were no good for the purpose at all. In trying to improve the situation it was found that an extract prepared from an active gelatin, when added to an inert gelatin, would make it photographically active. At this point the problem might have been considered as solved. But it was not. A group of experimenters at the Eastman Kodak Company then set out on an intensive campaign to find a more fundamental solution of the problem. The process of gelatin manufacture was subjected to a long and intensive study. As a result it was finally found that a derivative of mustard oil, the chemical name of which is allyl thiocarbamide, was the sensitizing substance. To render a gelatin suitable for making photographic emulsions, this compound has to be present to the extent of about one part in one million. But, if not present naturally, it can now readily be added, and added in just the proper amount.

The certain improvement that comes from research may readily be seen by comparing the first motor car with present ones; the first airplane with those of today; the first telephone system with an up-to-date one; the first electric motor with those in such universal

use to-day; the first motion picture with one of the current successes; the first radio with present ones; the first rubber tire, or even the second one, with the tire of to-day; or by setting Ben Franklin's printing press in a modern printing shop or newspaper plant.

Of the several other possible services which research can render, the following few may here be named in passing as supplementary to the improvement of products discussed above, and to the subjects covered later on:

Finding new products. Illustration: Research showed how to make edible fats from vegetable oils by boosting their content of hydrogen and so solidifying them through catalytic hydrogenation.

Lowering costs. Illustration: Scientific studies of combustion, kiln design, heat distribution, and temperature control reduced the amount of coal consumed in burning lime from one pound of coal for each three pounds of lime produced to one pound of coal per five pounds of lime.

Finding uses for by-products. Illustration: Research showed how to convert bagasse or sugar cane trash, which used merely to be burned, into a useful construction material that now has an important and profitable place in industry.

Supplementing failing resources. Illustration: When the supply of natural nitrates began to diminish seriously, research—and a truly monumental piece of research it was—showed how to produce nitrates in unlimited amounts by synthetic means, and right out of the air.

CHAPTER XXIX

INDUSTRIES ORIGINATED

THAT one-fourth of the nation's workers are now employed in industries which either were not in existence in 1900, or else were mere infants at that time, seems to be a conservative estimate. Directly and indirectly, the automobile industry gives employment to four and a half million persons, and the petroleum industry to about one million, the motor car having caused it to expand fifteen-fold since 1900. The American chemical industry, having multiplied its activities by ten in the present century, now employs over a million people. The electrical industry also is several times as large as in 1900, and gives employment to about a million persons. Largely on account of the demands of these newer industries, the iron and steel industry has expanded to several times its size in 1900, and in normal times it now employs nearly a million workers.

The motion picture industry, employing about three hundred thousand people, has grown up almost entirely during the present century. The radio and airplane industries, which together now give employment to one hundred and fifty thousand people, had not even been thought of in 1900. The telephone and the tele-

graph industries also have been so improved in usefulness of recent years that in extent of service they have grown by leaps and bounds until four hundred thousand persons are now required to maintain them. The number of telephones in use grew from one million in 1900 to nearly sixteen million in 1930, making a business that yielded over a billion dollars in operating revenue.

These altogether new or phenomenally expanded industries, together with those which supplement them in various ways and those which find their outlets among the workers in them, account certainly for one-fourth, and perhaps for as much as one-third, of the total number of persons employed in the country today. And every one of the industries named was founded by research. They have both come into existence and been constantly advanced through industrial research having acted as a translator of scientific facts to a using public. An official of the General Electric Company said in 1929 that one-fourth of its total production was then made up of new lines developed since the World War. Thus it is that, in the words of Arthur D. Little, "Research is the mother of industry."

The motor car, and therefore the industry arising from it, is said to have been made possible by three things: rubber, alloy steels, and petroleum. That is the truth, perhaps, but it is by no means the whole truth, for the scientific and practical developments that have made the up-to-date automobile possible came from a

great many sources. Electric lights, for instance, are certainly very essential to the utility of the motor car, even though they do glare at us too much sometimes. But without ductile tungsten it would not have been possible to put electric lights on automobiles. The spark plug is another small but necessary part of the motor car. And, had it not been for all the research that has been done in solving the chemical, metallurgical, and ceramic problems in connection with spark plugs, the gasoline engine of to-day would not be possible. Important also to the motor car are many other things. Some of these are alloy steels and the methods of making them—and ordinary steels as well—cheaply: aluminum, copper, lead, zinc, tin, nickel, and other metals; synthetic resins; synthetic finishes and the solvents for them; chemically coated textiles; electroplating; storage batteries; artificial abrasives; and materials of road construction.

Research lies back of every item in the list just mentioned. Aluminum, for instance, the production of which has become a large industry, was discovered by the German chemist Wöhler in 1828. In 1855 it cost ninety dollars a pound. By 1886, experiment had brought the price of a pound of aluminum down to twelve dollars. Then, in 1889, the application of the Castner process lowered the price to four dollars a pound. Meanwhile, in 1886, Heroult in Europe and Charles M. Hall in the United States discovered simultaneously that metallic aluminum could be obtained by

electrolyzing the aluminum clay called bauxite in a bath of fused cryolite. In 1895 production of aluminum by Hall's process was begun at Niagara Falls. By 1911 the production of aluminum had reached forty million pounds and the market price was down to twenty-two cents a pound. By 1929, as a result of continued experimentation and adaptation, the amount of aluminum produced had increased to nearly two hundred million pounds, seventy-five million pounds of which were used in making motor cars. And much more than that amount would go into cars, if only the price of aluminum were still lower.

"Chemical industry, as we know it," wrote Arthur D. Little, "may be said to have begun with the discovery in 1791, by the French chemist, LeBlanc, of a process which permitted the cheap production of sodium carbonate and caustic soda from common salt." And the chemical industry, thus started off just prior to the French Revolution by the experimental efforts of LeBlanc, has since been amazingly advanced by the constant researches of many thousands of men. According to a survey made by the National Research Council, the chemical industries even to-day spend more on research in proportion to their invested capital than any other American industry. Some one has pointed out the interesting fact that the industries which turned first to organized research and in which research has been most consistently prosecuted are those which have to deal with unseen or more or less obscure phenomena.

Instances of such industries are the chemical, the electrical, and the photographic.

One of the things Willis R. Whitney told the World Engineering Congress in Tokyo, Japan, in 1929 was what research has done and is doing to extend the uses of wood pulp. Although cellulose is an extremely complex material, men like Tollens, Fremy, Ost, Schwalbe, Chardonnet, and Cross and Bevan were brave enough to be pioneers in undertaking the study of it. Many chemists have devoted their lives to research on cellulose. "They purified it, they dissolved it. They tried every conceivable reaction with it. They hydrolyzed it, they nitrated, sulphided and sulphated it. They fermented it, they coagulated it, they precipitated it. They studied it as a colloid and they studied it as a crystalloid. They studied each new cellulose compound as to any possible interest it might have. They established an extensive literature covering several decades of careful observations."

As a result, it is not surprising that the usefulness of wood pulp cellulose has been greatly expanded. To-day "it is being applied for sound insulation, and for insulation of heat. It is used as a cotton substitute in high grade paper, and even modern silk is made of it. It is used in explosives, and also for non-inflammable movie films. It threatens to displace bone and ivory, except where they are actually born, and it is displacing the vehicles of varnishes and paints. These new cellulose products did not come through an increase in the rate

of grinding wood. They did not come because pulp tonnage produced new markets. They came because the researches of purely inquiring minds supplied new knowledge and altered points of view." They thus form an illustration of how research serves to establish altogether new industries, as well as to extend the services of already established industries.

From the viewpoint of effect upon employment there are two kinds of research: first, that resulting in the development of labor-saving machinery or of short-cut processes, with the inevitable elimination of some jobs; second, that resulting in the foundation of entirely new industries or in the extension of old industries, with a consequent increase in the number of available jobs. A great deal has been said and written about research as a job destroyer, but not much about research as a job creator. Even less has been said about what research might do to create jobs, if only a definite effort were made along that line. Careful analysis has shown that during the first thirty years of this century the job-creating effect of research just about balanced its job-destroying effect. And this was true in spite of the fact that no effort at all was directed specifically at creating jobs, whereas much was done in trying to eliminate jobs.

For the cure of such unemployment as arises from any cause, what appears to be demanded is an adequate program of research that is aimed directly at job creation, or at finding new services which people can per-

form for society. It is difficult to conceive of how the evil of joblessness can be corrected by any other means than through research, unless an evil can be depended upon to cure itself. Anything effective that is done will need to be preceded by patient study, and organized experimentation; and that is exactly what research consists of. The case appears to be somewhat parallel to the saying of some one that the only cure for the ills of civilization is more civilization.

"With the world's growing population," wrote Arthur D. Little, "the only remedy for unemployment is research. Were it not for the millions of opportunities afforded workers in the enterprises based on the contribution of science in years within the memory of most of us, we should now be faced in the United States with an unemployment problem appalling in its magnitude and heartrending in its results." Research even does something of itself to relieve unemployment. In terms of the number employed at it, industrial research is an industry larger than the telegraph business.

There are still further possibilities of expansion in industry through research, of course. The conditions under which we live are not yet so perfect that all the possible services to society are already being performed. Far from it. Of the other things that could be done, many, no doubt, have not even been thought of yet.

Charles F. Kettering has suggested that one of the industries of the future will be killing flies. As evidence in favor of this perhaps surprising prediction of his,

he mentions the fact that there are no more flies or mosquitoes outdoors in the sanitated parts of the Panama Canal Zone than there are on the inside of a well-screened house in the United States. If complete freedom from insects can be had right in the heart of the tropics, then why do we here put up with so many insect pests?

Besides the armies of insects that do so much to injure the comfort and the health of man and beast, there are all the parasites, such as boll-weevil, the corn borer, the fruit fly, the Japanese beetle, the gipsy-moth, and the like, that destroy the efforts of the horticulturist and the agriculturist. Surely it would be worth while to find out how to rid the country entirely of harmful insect pests, and then to set out and do it. The time may not be far off when we will have to, just as it was necessary to do in the tropics. Some of those familiar with insects have said that, unless something is done to control them, it is not at all certain whether man or insects will ultimately dominate the earth. Already we have let the insects destroy our chestnut trees, and grasshoppers sometimes devour the entire products of farmers' efforts over large areas.

We need better and cheaper houses for the average man, probably of a type that can be made in a factory where the cost of fabrication is low. Another desirable new industry is concerned with the cooling of houses, and with the conditioning of the air in them. We heat houses in winter when they get too cold for comfort.

Why should we not cool them in summer when they get too hot and humid for comfort? Theaters, stores, and trains are now cooled as a regular thing, and it is easier to cool a house than any of these.

Another needed extension of industry is the building of more and better roads, particularly secondary roads and express highways. Cars are regularly driven to-day on the open road at sixty miles an hour. Yet it is difficult to make a sixty-mile trip in less than two hours. The reason is that the roads we have now, even the main highways, are too nearly like horse-and-buggy roads. Rarely can a sixty-mile trip be made without passing through the main streets of several towns, and without being held up by traffic and by traffic lights. We need roads that go around all towns and clear of all obstructing traffic—roads on which a sixty-mile trip can be made in one hour instead of in two. We need highways on which five hundred miles can be driven in one day, and that without the expenditure of as much nervous energy as is needed to-day to drive half that distance. The improvement of secondary roads is something on which there is particular need for research, in order that much cheaper and better means of building and maintaining the lesser used roads may be developed.

As some of the other possible extensions of service to society, mention may be made of the need of improved sanitary facilities in cities and towns; of the elimination of noise; of the banishment of smoke and grime from

cities by eliminating, for one thing, the use of raw coal as domestic fuel; of the often-discussed need for reclaiming by reforestation much of what is now waste land, not to say wasting land; of the complete prevention or control of disease; of improvements in foods, and the emancipation of man from being, as he now is in large measure, a parasite upon the animal kingdom in respect to his food supply. In our eating, as Mr. Kettering has said, we humans are about the lowest—or perhaps the highest—form of animal parasite. We prey upon animals. We rob nests. Our behavior is typified by the fact that we steal the eggs from the hen, and then we cut off her head and eat her too. While we are doing this, we are putting up with inferior foods in some instances, because they have not been made especially to fill our needs at all.

Since research has been so helpful in solving problems in other fields, it is nothing more than a natural suggestion that it be applied to people's employment problems, to the creation of new industries and therefore of new jobs by the extension of needed services to society.

CHAPTER XXX

INDUSTRIES DESTROYED

A MILLION acres of land in India used to be devoted to raising the indigo plant. In 1897 the value of the crop was about \$20,000,000. Seventeen years later, in 1914, the annual production had dropped down to one and a half per cent of that figure, or \$300,000 worth. The reason for the decrease was not a blight of some kind, nor a reduction in the demand for indigo. It was because indigo was then being artificially fabricated from a constituent of coal tar. Adolf von Baeyer and his helpers, as a result of a fifteen-year research supported by the Badische Anilin und Soda Fabrik at a cost of \$5,000,000, had made the synthesis of indigo from benzene, via aniline, practicable. They had made it so entirely practicable that 96 per cent of the world demand was being produced by their process. This is a classical example of research as a destroyer of industries.

But there are other instances of a similar kind. One of these is the wagon and carriage manufacturing industry. In 1900 there were 62,500 persons directly employed at making wagons and buggies. Now, thanks to the motor car, there are less than five thousand. The "thanks-to-the-motor-car" interjection is justified, be-

cause there are ten times as many people directly employed at making automobiles as were given employment in the wagon and carriage manufacturing business in its most flourishing period. More than one carriage-making shop grew up into an automobile factory.

The motor bus has almost put the electric traction line out of business. In Michigan, for instance, there were only three interurban traction lines left in 1930. The motor bus and the automobile together have caused a great drop also in the local passenger traffic of railroads, as well as some reduction in their longer hauls. When "Colonel" Drake struck oil at Titusville in 1859, there are said to have been more than fifty companies in the United States engaged in making "coal-oil" from coal or from oil shale for use in lamps. Those companies had already by that time practically annihilated the American whaling industry. And they, in turn, soon were either forced out of business, or else they turned to the making of "coal-oil" or kerosene from petroleum.

Chemical industries in particular are subject to destruction, or to reduction, by the coming of some new development. Castner had only got his sodium process for making metallic aluminum well into operation in Birmingham, England, when along came the electrolytic process of Hall and superseded it completely. The making of ammonia and nitric acid out of the air is rapidly superseding the immense business of getting

it from Chilean nitrates. New applications of synthetic chemistry have been coming along in rapid succession, each one upsetting to a greater or lesser degree some established business. We now have synthetic methanol, synthetic ethyl alcohol, synthetic ethyl ether, synthetic acetone, synthetic acetic acid, and so on. As was said by *The Saturday Evening Post*, "The march of science, discovery, and invention has been so speeded up that the advances of any year may forecast the early supersession and abandonment of the old familiar ways of doing things and the old equipment employed in doing them."

Not always, though, does the new thing completely displace the old. It often reduces its use. But in some cases the new may not even do that. Neither the automobile nor the tractor has yet thrown the horse out of a job. In 1900 there were 21,500,000 horses and mules on American farms. In 1930 there were still 18,750,000, or five out of six of them, left. Neither the gas light nor the incandescent electric bulb has yet run the manufacturer of kerosene lamps out of business. About twice as much kerosene was produced in 1929 as in 1899, and one up-to-date manufacturer of kerosene lamps has been advertising them over the radio. The telephone did not displace the telegraph, nor has the radio run either one of them out of business. These three means of communication supplement and assist one another in helping to make a smaller place out of the world. The telephone has become an essential ad-

junct of radio broadcasting, and the radio in turn is used in trans-oceanic telephony.

The introduction of glucose by the candy makers did not hurt the sale of sugar for candy. What it actually did was to boost it. Synthetic perfumes have not driven out natural perfumes. On the contrary, they have stimulated the growing of flowers for making the needed essences. The tonnage of the world's sailing vessels is twice as large now as it was when the steamship was introduced a century ago. And, as Willis R. Whitney has said, we are still even making tintypes, tallow candles, ox-yokes, and tin bathtubs.

Much was written a few years ago about what the coming of synthetic methanol did to the hardwood-distillation industry. But that industry still survives on a considerable scale. The reason is that there are markets for "wood chemicals" other than methanol.

Charles F. Kettering tells of having been present at a meeting of electrical men several years ago when a speaker, in giving an address on the competition of the Welsbach gas mantle with the incandescent electric lamp, said that what the electric light industry needed most for meeting that competition was a more efficient lamp. Strong exception to this view was taken by a subsequent speaker, who presented figures which appeared to show with conclusiveness that, instead of helping the electric light business, such an improvement would probably so reduce the amount of current required, and hence the income of the industry, as

to ruin it altogether. Soon after that, incandescent electric lamps of very much higher efficiencies did come, and every one knows the result. The consumption of electricity did not go down. On the contrary, it mounted upward at a very rapid rate, as users of light began to make the nights brighter and brighter.

Bramwell in England predicted in 1881 that the gas engine, just then coming on the scene, would displace the steam engine. "I very much doubt," said he, "whether those who meet here fifty years hence will then speak of that motor (the steam engine) except in the character of a curiosity to be found in a museum." Later Bramwell left some money with the British Association to be paid in 1931 as an honorarium "to a gentlemen to be selected by the Council to prepare a paper having my utterances in 1881 as a sort of text, and dealing with the whole question of prime movers in 1931, and especially with the then relation between steam engines and internal combustion engines." That lecture was delivered in 1931 by Sir Alfred Ewing, and he found the use of steam increased, not decreased. This was so in spite of the fact that the internal-combustion engine had found a most important and essential place in the picture, a place that Bramwell never dreamed of.

So it is that, as a result of new developments which have arisen from research, comparatively few industries have actually disappeared. In the main, what has happened rather has been that affected industries have been

greatly changed or reduced in scope. Hence, one of the principal services of research to such industries is to prepare them for those changes which are inevitable, and to guide them in properly adjusting themselves to the changes as they do come along.

Charles F. Kettering has given the following as one definition of research: "Research consists in finding out what to do when you can't do what you are doing now." And certain it is that one of the principal services of research to some industries is in saving them from destruction. The late John E. Teeple pointed out that of the forty-four American companies which produced potash during the World War, only one survived; and that company happened to be the only one which had under way a program of research—or was that an accident?

The wise investor nowadays does not base his estimate of the soundness of an industrial enterprise altogether upon its financial statement; for that is only a record of past performance, and it may be a measure to some degree of mere luck. But instead—or rather also—he investigates what, if anything, the enterprise is doing in the way of forward-looking research to insure that it will continue to occupy a place of service in a changing society, and so to guarantee its permanence and prosperity.

The Victor Talking Machine Company used to be so prosperous that for eleven years its common stock dividend averaged more than forty-two dollars a share.

In 1922 it paid in addition a stock dividend of 600 per cent. But meanwhile out of the world's research laboratories came radio, and very soon the Victor Talking Machine Company was paying no dividends at all. But in this extremity they did a wise thing. They turned for help to a good research laboratory, with the ultimate result that before long their products and their consequent sales were so much improved that a resumption of dividends became possible. Such an employment of research for the preservation of an industry by keeping its products abreast of developments made by research in other fields is perhaps a sort of "diamond cut diamond."

Thus it happens that the cost of research may be of two kinds. There is, first, the cost of doing it, and there is, second, the cost of not doing it. And the cost of not doing it may be—it is indeed quite likely to be—much the greater of the two.

CHAPTER XXXI

DIVIDENDS: ECONOMIC

THERE is a Chinese proverb which says that he who holds the iron of the world will rule the world. Like many another proverb, that saying is only a half-truth. Natural resources are of no value unless, or until, their possessors learn how to use them in the service of society. And China itself is one of those countries which, as Sir Richard Gregory suggests, has left its talents buried in the ground.

With plenty of coal and iron and other minerals underlying their fertile soil, the Chinese people have shivered and starved through many, many centuries. For no reason other than failure to make use of their natural wealth, untold millions of Chinamen have died from famine or drowned in the periodic floods of their great rivers. What an immense amount of suffering could have been saved—could still be saved there, for that matter—by a little application of the knowledge that research has yielded on agriculture, on flood control, and on transportation.

It seems to be a safe statement that every advantage, economic and otherwise, that people to-day have above the savage came as a result of research—research being defined in the broad sense of the finding of new

knowledge or of the improving of something. It was research of a kind that made the Bronze Age succeed the Stone Age and which made the Iron Age in turn supersede the Bronze Age. Every single advance that has ever been made in any of the arts came as a result of somebody's experiment or of some observing person's discovery.

Research, whether organized or unorganized, conscious or unconscious, has benefited people from an economic viewpoint in three respects. The first of these, and perhaps the most important of them, is the one already suggested, namely, the ability it has given us to have comforts and advantages which we could not otherwise have had. The second is that the enlargement of the scope of industry, or of the forms of service to society, resulting from research has made more work for people to do. That is to say, research has created jobs. The third economic benefit conferred upon people by research lies in the improvement of commodities from the viewpoints at once of the degree of service they render, and of the lower prices at which they can be bought. All three of these ways in which research has benefited people can be illustrated by mentioning developments in one single field, that of transportation and communication.

In the *Outline of History*, H. G. Wells said that after the defeat of Napoleon's army in Russia, it took him a total of 312 hours, or thirteen twenty-four-hour days, to travel the 1400 miles back to Paris from Vilna. Sped

forward with every advantage of those times, he still averaged less than five miles an hour. An ordinary traveler could scarcely have covered that distance then in twice the time. Those were about the maximum rates of travel as were possible between Rome and Gaul in the first century A.D.

But just a generation later than Napoleon's day came a great change, a change that not only divided by ten the time required for the ordinary traveler to cover such distances, but that also immeasurably improved the comfort with which the traveling was done. That change was the railroad.

How much the railroad and the many other improvements in transportation and communication, such as the telegraph, the telephone, and the motor car, which succeeded the railroad and which were all based upon research, have meant to people from an economic—not to say from a social—viewpoint it is not possible to compute. Without modern means of transportation and communication, how could this immense country of ours have been opened up agriculturally, industrially, and socially, to become, as it has, one of the best places in the world in which to live? So far as ability to go from place to place or to move products from one point to another is concerned, it is not as far from New York to San Francisco to-day as it was from New York to Buffalo a hundred years ago.

Some idea of what the transportation and communication industries have done in the second respect,

that of creating jobs for people, may be had from the fact that to-day at least one American worker in every five is employed directly or indirectly in the transportation and communication industries. Included in this group are the one and one-half million workers on railroads, the one million in the oil industry, the one million people working on the roads, the two million making the materials and equipment used in road building and maintenance, and the four and one-half million workers employed directly and indirectly in the motor car business, besides the other millions who are given work by the telephone, the telegraph, the radio, the street railway, the subway, the airplane, and so on.

An illustration of the third form of economic benefit derived from research, or that which comes from the simultaneous improvement of a product and reduction in the cost of it, is offered by the automobile. Thanks to a constant striving after improvement, the motor car of 1935 as compared with the 1913 car is a very much better and more serviceable vehicle, not to say a much more beautiful and comfortable one. Yet at the same time, thanks again to consistent improvements in means of fabricating them, the cost of a car in the lower-price bracket has been reduced at the same time from fifty cents per pound to twenty cents. That is to say, research has expanded the automobile-buyer's dollar to more than twice its former size, while at the same time it has enabled him to buy a car the like of which

in comfort, convenience, and service, could not have been had twenty years ago at any price.

In agriculture, research has taught us, as H. G. Wells said, "so to fertilize the soil as to produce quadruple and quintuple the crops got from the same area in the seventeenth century." Another great aid to this increase was the breeding of better plants based upon the knowledge of the laws of heredity in plants, which knowledge grew out of the pioneer work of Abbot G. J. Mendel, that Austrian monk who found that the peas in every pod are not alike. It is because of what his research, and all that done by subsequent investigators, has taught us about heredity and selection in plants, and in animals as well, that, as Arthur D. Little says, "the cotton spinner has better grades of cotton; the beets coming to the refinery carry 18 per cent of sugar instead of 6 per cent; tobacco plants grow 30 per cent more leaves fit for wrappers; wheat has more gluten; corn and potatoes more starch, hogs are heavier, cattle bigger, and hides larger and better."

It is altogether without people realizing it that research has made possible some of the things we have. The Panama Canal is an illustration of this point. The French with well-engineered plans, with plenty of money, with the best of machinery, and with all the help needed, gave up as hopeless the job of digging a canal across Panama. But the Americans with nearly the same plans, with comparable machinery, and with no more adequate financing or help, succeeded in do-

ing it all right. The one outstanding difference between the two groups was that, thanks to much intervening research, we happened to have the advantage over the French of knowing the habits of two mosquitoes—of knowing that one of them carried malaria and the other yellow fever. So we knew that the thing to do before even trying to dig the canal was to clear the territory of those interfering insects. It was because we did that, and not because we had any greater abilities than the French as canal-diggers, that there is now a canal across the Isthmus of Panama.

It is to the ordinary man that research has made its greatest contribution. Research has given the ordinary citizen things that only the very wealthy used to be able to afford. Research has done even more than that. It has given the ordinary man advantages that formerly could not be bought with all the wealth of Cræsus. It was only a few years ago that none but the very wealthy could afford a bathroom, for instance. Queen Victoria did not have a bathroom in Buckingham Palace. Thirty years ago the man who had an automobile was on a par from the viewpoint of transportation with the man who had a chariot thirty centuries ago. Now nearly every family has a car, even the worst of which is so far ahead of the chariot—or even of the early automobile, for that matter—that the carriage of the ordinary working man is now one the like of which the ancient king never even dreamed of having. But neither could the ancient king command a tele-

phone, a radio, an electric refrigerator, nor any of the many other electric servants. He did not have brightly lighted houses and streets, nor railways, nor airplanes, nor motion pictures. That the ordinary man to-day does have the use of all these comforts came about altogether as a result of research. And it is these services, economic and otherwise, that research has performed for the ordinary man which constitute its great usefulness to the world.

CHAPTER XXXII

DIVIDENDS: EDUCATIONAL

I will mention some of the gains which the scientific temper has brought us. Even in politics and religion, where passion and prejudice are most potent to obscure the intellect and distort the judgment, there is a higher standard of veracity and more respect for evidence. Rhetoric and advocacy are distrusted. The scientific spirit has transformed history, and has imposed rather more conscientiousness even upon controversial literature and public speaking.

Curiosity, which was condemned by monkish morality, is now praised, as it was by the Greeks. To seek for the truth, for the sake of knowing the truth, is one of the noblest objects that a man can live for.

THE foregoing quotation in regard to the educational value of research is not an expression of the opinion of some research worker or man of science, who might be said to have "an ax to grind." It is instead the word of a distinguished, and sometime critical, clergyman, William Ralph Inge, Dean of St. Paul's, London.

Edgar Allan Poe lamented that science had driven the hamadryad from her grove. What he meant was that science had destroyed people's sense of the mystery, and hence of the wonder, of nature. It is true, as Sir Richard Gregory pointed out, that people no longer

believe in the existence of such creatures as gryphons, harpies, phœnixes, and rocs. They do not believe any more that the crocodile weeps after it has eaten a man; that when a horsehair is left in a stream it turns into an eel; nor that during the winter swallows lie hidden at the bottoms of lakes and rivers, two together, mouth to mouth, and wing to wing. People do not believe, as they did before Newton, that comets are heralds of disaster; nor do we to-day burn people to death for daring to inquire into the mysteries of the human body, as was done to Servetus.

The truth of the matter is that the ignorance of nature upon which rested the sense of mystery that Poe deplored the passing of was accompanied not so much by admiration and love as by fear and dread. Just in proportion as through research our knowledge of nature and her laws has been extended has the gloom of ignorance, superstition, and fear been dispelled from the minds of people. "Know the truth, and the truth shall make you free," are words that apply with particular force to what is commonly called scientific truth.

As R. A. Millikan says in *Science and the New Civilization*, "The disasters that can befall mankind merely because of erroneous conceptions of the nature of the world in which we live are all illustrated by the historic record of the miseries that came upon the earth in the year 1000 A.D. because of the widespread belief that the world was coming to an end at that time." But science

has since shown that the world is already at least a billion years old and that man has perhaps another billion years ahead of him, in which, as Dr. Millikan suggests, he may learn to live a million times more wisely than now.

It was science that taught us not to blame epidemics of disease upon Providence as visitations of wrath on account of human iniquities. No longer do people try to ward off scourges of the Black Death, "the pestilence that walketh in darkness," by hitching four widows to a plow and drawing a furrow around the town in the dead of night, as used to be done in Russia. The reason is that two inquisitive Japanese doctors, Yersin and Kitasato, discovered that in reality the Black Death comes by no more supernatural means than rats and the jumping fleas that infest them.

As for the results of research having destroyed people's sense of wonder at nature or of admiration for her marvelous processes, it is really just the inverse that has happened in some instances. This is one of the cases in which it is particularly true that truth is stranger than the fiction of people's imaginations. How much of a surprise it must have been to those who discovered it, and how wonderful to those who now know about it as a result of the discovery, that the trees in the grove, which the hamadryad used to inhabit, get their principal sustenance not from the soil in which they are rooted but out of the air in the form of the carbon dioxide, which is present there in minute concentra-

tion—in a concentration still more minute than that of the “rare” gas argon. But the mystery of vegetation still persists, for no one yet knows just how it is that the tree can take carbon dioxide out of the air and water out of the soil and weld them together to form the complex substances that the beautiful tree is made of.

Surely the wonder of the bee was not reduced when Charles Butler found that the so-called “king of the bees” was in reality a queen, and that whether a female bee becomes a queen or just a worker is determined by the kind of food it is fed in the larval stage. The mystery and wonder of nature was not reduced when the English surveyor, William Smith, showed that each formation of rocks could be identified by the fossils it contained. Camerarius did not reduce human admiration of nature when he showed that plants have sex and that fertilization is accomplished by means of pollen. The fifty years which Fabre devoted to the study of living insects in southern France, and the fascinating stories he wrote about what he had observed, did not reduce admiration of nature to any degree, but on the contrary added immensely to it.

Michael Pupin has pointed out that every cultured person is expected to have an intelligent view of literature, the fine arts, and the social sciences. But who, he asks, has ever thought of suggesting that culture demands as well an intelligent view of the primary concepts in the fundamental sciences? There would be

more straight thinking about things in general, Dr. Pupin suggests, if people had such knowledge. "In the long run," wrote Forest Ray Moulton in *The Nature of the World and of Man*, "it will probably be found that the greatest benefit of science to the human race has been not in providing the material things of life, but in furnishing unlimited opportunities for cultivating the intellectual."

If the scientific attitude were generally applied, says Professor John Dewey, "it would liberate us from the heavy burden imposed by dogmas and external standards. Experimental method is something other than the use of blow pipes, retorts and reagents. It is the foe of every belief that permits habit and wont to dominate invention and discovery, and ready-made system to override verifiable fact."

Some of the most useful discoveries [to quote R. A. Millikan again] have exerted their chief influence, not through showing how the yield of beans or cabbages per acre could be doubled, but rather through preventing mankind from wasting its precious energies on useless undertakings, such for example as building a tower of Babel. All knowledge that helps toward an understanding of the nature of the universe of which we are a part is useful, for we need very much more of it than we now have, or shall have for centuries to come, to enable us to direct our energies toward wise effective living instead of wasting them on beating tom-toms, inventing perpetual motion machines, or chasing either physical or social rainbows.

CHAPTER XXXIII

DIVIDENDS: HUMANITARIAN

THAT the results of research are altogether materialistic and without any real, fundamental benefit to people is the belief of some. Gilbert K. Chesterton says that science is "a thing on the outskirts of human life—it has nothing to do with the center of human life at all." The estimate of John Jay Chapman—which we hope is an underestimate—is that "Science...is really a branch of domestic convenience, a department for the study of traction, cookery, and wiring." The railroad was only a generation old when Thoreau warned, "We do not ride on the railroad; it rides upon us." In *World's Work* an editorial called "Honoring Mr. Edison" said that with all the gifts of such men "we do not seem to ourselves to be much better or happier than our ancestors were. It sometimes appears that they have done little except enable us to run around a little more rapidly in our squirrel cage of civilization."

It may be that these negative views about the fundamental value of research arise in part from the inherent human dislike for change, even though the change be sometimes for the better. The oldest known piece of writing in the world is said to be an expression of this feeling. It is a piece of papyrus in a Con-

stantinople museum which says, "Alas, times are not what they used to be." And even in these modern days the mental attitude of many people is typified by a Do-Not-Disturb sign hung on the outside door knob.

In science, progress is necessarily slow. Hence, to judge the value of it, one has to stand back far enough to get its results in proper perspective. Too short a view may be quite misleading.

Arthur D. Little in his address "The Fifth Estate" imagined the feelings of Benjamin Franklin if he could but return to-day. One of the things that would surprise Franklin most would be what science has been able to do since his time to relieve human misery.

In great hospitals, permeated with the scientific spirit and equipped with many new and strange devices for the alleviation of human suffering, he would hear of the incalculable benefits which medical and surgical science have conferred upon mankind. He would see the portraits and listen to the story of Pasteur and Lister and Loeb and Ehrlich.... We can little realize the emotion with which one like Franklin would learn in a single afternoon of the germ theory of disease, of preventive serums, of antisepsis, of chemotherapy, of the marvelous complexity of the blood stream, and the extraordinary influence and potency of the secretions of the ductless glands. What appraisal would he make of the service to humanity which, in little more than a generation, has mitigated the horrors of surgery by the blessings of anesthesia and asepsis; which has controlled rabies, yellow fever, typhoid fever, tetanus; which is stamping out tuberculosis, curing leprosy, and providing specifics for other scourges of the race.

The records show that at the time of Benjamin Franklin's birth three out of four babies born in London died in infancy. And Dr. H. W. Haggard has told how Louis XIV, who lived just a few years before Franklin, and who was the healthiest ruler in Europe in his day, had the following ailments: smallpox, typhoid fever, a venereal infection, measles, tapeworm, abscessed teeth, gout, a dislocated elbow, "a most personal but unmentionable complaint," malaria, a carbuncle, and hardening of the arteries.

Charles Darwin set out in life to become a physician. But he gave it up because, being then before the days of anesthesia, the horrors of the operating room were too great for one of his sensitive nature to stand. One of the operations he attended as a medical student was on a little child. Darwin could not bear the anguish, but rushed out before the end of the operation to give up altogether his aspiration of becoming a physician. But, since science has conferred upon humanity the boon of anesthesia, "The fierce extremity of suffering," in the words of Oliver Wendell Holmes, "has been steeped in the waters of forgetfulness, and the deepest furrow in the knotted brow of agony has been smoothed forever."

Before the researches of Lister, which showed surgeons how to perform operations in such a way as to keep out infection, eight people out of ten died following operations in some hospitals. The great Pasteur, who through his marvelous contributions to the under-

standing and control of germ diseases became one of the greatest benefactors of mankind that ever lived, had his own life saddened by the loss of two of his children from typhoid fever. Thus was the greatest of all warriors against microbes robbed of priceless possessions by a kind of microbe that he did not know about.

Lord Macaulay, for one, would probably not agree with Chesterton that science is merely a thing apart from the center of human life. Speaking of the death of Queen Mary in 1694 from smallpox, Macaulay said in his *History of England*:

That disease over which science has since achieved a succession of glorious and beneficial victories was then the most terrible of all the ministers of death. . . . Smallpox was always present, filling the churchyards with corpses, fermenting with constant fears all whom it had not yet stricken, leaving on those lives it spared the hideous traces of its power, turning the babe into a changeling at which the mother shuddered, and making the eyes and cheeks of the betrothed maiden objects of horror to the lover.

Only fifty years ago diphtheria killed six times as many babies as it does now. It was even then not yet known that typhoid fever comes from microbes in unclean drinking water or milk, nor that malaria and yellow fever are spread by mosquitoes, bubonic plague by a flea, and typhus by a louse. The time had not yet passed when, as Herbert Hoover said in his address honoring Dr. William H. Welch, Providence was often made responsible for the fate of man, "rather than the

bacillus which should never have been allowed to infect him." Neither the X-ray nor radium had yet been discovered. Insulin for diabetes, emetine for dysentery, salvarsan for syphilis, were not yet known; nor were thyroxin, adrenalin, or vitamins.

If any one of the skeptics about the real worth of scientific exploration had been on his death-bed at the end of an average lifetime fifty years ago—to borrow an illustration from C. E. A. Winslow in *Whither Mankind*—what would have been his feelings if the personification of Science had appeared and offered to prolong his life twelve years more? Yet, without any dramatics at all, that is just what has actually happened; for, thanks to a great deal of persistent but quiet research, the span of life to which every one on the average may look forward at birth has been increased 30 per cent just within the past half-century. Surely it must be that, in spite of what critic Chesterton has said, the marvelous effects of some of these results of research do come right close to the actual center of human life.

A very large part, if not the greater part, of all that has been done to soften the troubles of the world—darkness, cold, hunger, slavery, pain, sickness, death—has come out of research. Speaking of one of Edison's many contributions to society, the incandescent lamp, the editorial in *World's Work* referred to in the opening paragraph had this to say:

The spreading of light is symbolical of the best of modern life. Our ancestors, in caves or huts, were prisoners of the

dark. Out of the dark came the grotesque superstitions and fears that weighed them down and made their lives wretched. But our age is conquering the darkness, both physical and mental.

Even as recently as 1850 most people had no other light at night than that given by tallow candles or sperm oil lamps. Coal-oil had become available not long before that, but it sold at about a dollar a gallon, a price prohibitive for most people of that day.

The specter of hunger is an ancient one that has been banished by science wherever the results of science have been made use of. "For most of the human race, the world of two hundred years ago," said Peter Molyneaux, "was a world of extreme poverty, of bare subsistence, and a world of almost unceasing and very hard labor.... It was a world of scarcity and constant threat of famine." But, as was said some time ago in the *Monthly Labor Review*, "We have long since lost all fear concerning our food supply." That is true, though, only in some countries, and it is true there merely because of what research has done to make it so. Were it not for the contributions of research in the form of fertilizers, better and more prolific plants, systematic crop rotation, and improved method of agriculture in general, there would probably be much real starvation in countries other than those, such as India and China, where the people do not know about modern agricultural methods. What India needs, said Rabindranath Tagore, is the scientific learning of the West, so that

the people may learn how to support themselves and end the fear of starvation. That was said by an outstanding citizen of a country in which sixty million people go to bed hungry every night, and in which seven and a half million die every year from undernourishment or from outright starvation.

Thomas Robert Malthus prophesied at the opening of the nineteenth century, not at all without reason, that population would soon exceed the available food supply. And even as late as the opening of the current century, Sir William Crookes, in addressing the British Association for the Advancement of Science, said, "England, and all civilized nations, stand in deadly peril of not having enough to eat." What Sir William was referring to was the then threatened early exhaustion of the world's natural stores of nitrogen or nitrates. And, as was shown in 1840 by Justus von Liebig, the pioneer of fertilizer chemists, nitrogen along with potash and phosphorus are absolutely essential to crops. But it was just a few years after Sir William issued his warning that science contributed to the world a complete and permanent solution of the problem of nitrogen. This it did by showing how to convert into forms usable as fertilizer the inexhaustible store of nitrogen in the air. So it is that, thanks to research, neither the prophecy of Malthus nor the prediction of Crookes have come true. But, on the contrary, farmers are often in the dumps because they have grown a great deal more in the way of crops than people can use. If

any one is hungry to-day, it is the fault of something other than science.

Slavery of many kinds is also a thing that has been abolished by the results of research. "The Roman civilization," said H. G. Wells, "was built upon cheap and degraded human beings; modern civilization is being rebuilt upon cheap mechanical power." The major accomplishments of research workers, in the words of Herbert Hoover, "are the rivers of sweat saved from the backs of men and the infinite drudgery relieved from the hands of women." Galley slaves are no more. They have been emancipated by the inventor of the steam engine and the oil engine. "Fourteen hours of labor in which women and children were forced to share," said J. McKeen Cattell, "formerly provided only hovels, lice and black bread for most people and luxuries for a very few. But now, owing entirely to science, seven hours of labor will supply comfortable homes, warm clothes and healthful food for all. If the resources provided by science were properly distributed, that is, if we had an adequate applied science and economic psychology, there is now sufficient wealth to enable all to share...in the most nearly ultimate goods of life, namely, friends, things to do, freedom, self-respect." Will Rogers, speaking of this same matter of the imperfect distribution of things that we have in great plenty, said, "We are the first nation to starve to death in a storehouse that's filled with everything we want."

"To me that civilization is materialistic," said the Chinese scholar, Hu Shih, "which is limited by matter and incapable of transcending it; which feels itself powerless against its material environment and fails to make the full use of human intelligence for the conquest of nature and for the improvement of the conditions of man." So speaks even an Oriental. And the English woman preacher, A. Maude Royden, said of the effort to relieve certain distresses arising out of poverty in China, "I think that the scientist who is endeavoring to alleviate the poverty of China is attending to a weightier matter of the law than those Christians who can only pick up the broken human beings."

It is to be expected that research will continue to produce still more and more in humanitarian dividends. There is certainly much yet to be done along many lines, such for instance as in the further banishing of needless pain and disease from the world. Although a great deal of progress has already been made in curing and preventing disease, it is really nothing but a mere beginning. "Let us not deceive ourselves," wrote Arthur D. Little in "The Fifth Estate," "human life is still a hard and fearsome thing. Mankind is required to maintain existence in a world in which, as Kipling has said, 'any horror is credible.'" There are many of the several thousand human diseases that are still called incurable; but, as Charles F. Kettering has said, calling a disease incurable is only a euphemism. It is merely a way to avoid saying that we do not know how to cure it; for

the disease has no objection to being cured, if only we knew how. And we must find out.

Benjamin Franklin, in a letter which he wrote from Paris to that pioneer chemist and discoverer of oxygen, Joseph Priestley, expressed something of his belief about what science would later do in giving man power over matter: "Agriculture may diminish its labor and double its produce; all diseases may by sure means be prevented or cured, not excepting that of old age, and our lives lengthened at pleasure even beyond the antediluvian standard." To this Franklin added a fervent wish that something of the same thing might happen also in respect to the mastery of man over the spirit: "O that moral science were in a fair way of improvement, that men would cease to be wolves to one another, and that human beings would at length learn what they now improperly call humanity!"

Fortunately for society, the doubts of some people about the real worth of scientific research will not stop the up-to-date explorers who are doing their level best to "Ring out old shapes of foul disease," and all the other evils of mental darkness. Each step, however small, that they are able to make toward divorcing the word "incurable" from every disease and the adjective "unpreventable" from every difficulty will be a boon to the world and will help still further to justify research.

PART VI

MISCELLANEOUS

IN a letter to Lord Rosebery, William E. Gladstone quoted with approval Mrs. Grote's saying that politics and theology are the only two really great subjects. Research is thus not the greatest subject in the world perhaps. But still it is altogether too big a one to be covered completely by what little has been said about it in the five preceding parts of this work or even by ten times that amount. So, in order to round out this treatise a little better, a few of the miscellaneous phases of research are discussed here as Part VI.

CHAPTER XXXIV

TRUTH

WHAT is truth?" That question is a baffling one which has been asked in every age. Nineteen centuries ago it was put to his most famous prisoner by Judge Pilate, the Roman. Five centuries still before that time, Confucius made the observation that "Buried deep truth e'er lies." The question about what is truth is still not much less of a puzzle to-day than it has always been, of course. Truth, in most things, is both so hard to find and so hard to recognize even after it has been found that William Hazlitt said in *The Spirit of the Age*, "One truth discovered is immortal, and entitles its author to be so." Truth is often such an unusual thing that Bernard Shaw also could say in *John Bull's Other Island*, "My way of joking is to tell the truth. It's the funniest joke in the world."

Happily, the question about what is truth is one that inquisitors of nature are not puzzled about to the same degree; for truth in science is what nature reveals about herself. Nature, like figures, does not lie; although it is sometimes so hard to wring the truth out of nature that Charles Darwin once jokingly said to Wilder D. Bancroft, "She will tell you a direct lie if she can." But in this same connection Lord Kelvin said more soberly,

"There is one thing I feel strongly in respect to investigation in physical or chemical laboratories—it leaves no room for shady, doubtful distinction between truth, half-truth, whole falsehood."

The problem of the research worker is not to make sure that nature will tell the truth; but it is, first, to devise such means as are necessary to wring definite and explicit answers from her, and, second, to know whether his experimental conditions are such that the result obtained really represents nature's reply. Once nature's response has definitely been received, the truthfulness of it can be depended upon. If an explorer who has set out to question nature returns with an incorrect answer, it is either because he did not understand nature's response, or because the technique of his inquiry was so faulty that the answer he received was only static, and not a message from nature at all.

But, although if properly questioned nature can always be depended upon to tell the truth, there is such a thing as getting her to tell the truth without telling the *whole* truth. For many years nature told every experimenter that pure soft iron was a more magnetic substance than iron alloyed with other magnetic metals, such as nickel for instance. And so it happened that pure Swedish or Norway iron has always been one of the standard materials for making telephone, telegraph, and other electrical apparatus.

Then one time, not so long ago, G. W. Elmen, an experimenter in the Bell Telephone Laboratories, set out

to question nature more minutely, in order to discover whether she had been telling the whole truth about the magnetic properties of iron, or whether she had not been holding something back on every one who had questioned her before. The reason he wanted to do this was that the speed of submarine cables is limited by the magnetic permeability of the metal used for "loading" them. The magnetic permeability of pure iron, although the best of any material known at that time was so poor that not only were the cables altogether too sluggish to handle the multitude of telegraphic messages that people wanted to send through them, but also they could not possibly be used for telephoning across the seas.

The question investigator Elmen asked of nature was this: "If one starts with perfectly pure iron and begins to add nickel to it, and keeps on adding nickel to it, will a point be reached where the magnetic properties will be improved instead of made worse?" So he began to alloy nickel with pure iron, a little more each time, and then to measure the magnetic qualities of each alloy. But nature stuck persistently to her story, and with each successive addition of nickel the magnetic permeability kept going down instead of up. Twenty per cent of nickel was reached, and still the curve bent lower and lower, until magnetic permeability had fallen to less than one-half that of pure iron. Surely it looked as though nature's reply was being firm enough and consistent enough to convince any one that add-

ing nickel to iron does not improve its magnetic qualities.

But, although explorer Elmen was down in the valley just then, he was on the verge of a great discovery. When he had gone on a little further to an alloy containing just under 25 per cent nickel, the curve of magnetic permeability changed direction and began to go up, and kept on going up and up, until at 40 per cent nickel and 60 per cent iron it had become five times as high as for pure iron. Still researcher Elmen went on, but for a while with no further improvement. Then, just above 55 per cent nickel, the magnetic permeability of the alloys began to shoot up again. Up and up it went, until at 78.5 per cent nickel and 21.5 per cent iron a level of permeability was reached that was "more than thirty times the corresponding value for the best soft iron." Then, upon still further addition of nickel, the magnetic permeability began to fall off rapidly, until at 100 per cent nickel it had reached the very low value corresponding to nickel alone.

So by a minute and persistent cross-examination of nature, carefully quizzing her every inch of the way—or by a sort of scientific third degree—detective Elmen had found that nature had not before been telling the whole truth about the magnetic properties of iron. And she had admitted to him that, if the copper submarine cable were "inductively loaded" by being surrounded with a shell of this new alloy of iron and nickel, its current-carrying qualities would be so greatly

improved as to permit messages to be transmitted over it at speeds many times those possible before.

But nevertheless, as she always does, nature had given a truthful answer to every question about the magnetic qualities of iron that had been properly put to her up to that time. Previous investigators had simply not taken the trouble to quiz her minutely enough. So the problem of the research worker has not necessarily been solved when he first wrings from nature a reply to some question of his. The answer may be only a part of the truth.

"Scientific research," it was said in a recent publication of the Metropolitan Life Insurance Company, "is like the exploration of a strange river in an unknown country. We pass one bend and we learn that the river reaches a certain point flowing from the east. That much is gained. Then we push on and we find that beyond another turn our river which has seemed to come from the east is really formed by two tributaries entering respectively from the north and south. The first observation was correct. We do not have to unlearn what we have learned but to add new knowledge to the old."

Nature placed on the witness stand does not need to be sworn in. She will tell the truth, and nothing but the truth. But as for getting her to talk in the first place and then to tell the *whole* truth, those are often very difficult matters indeed.

CHAPTER XXXV

BY-PRODUCTS

WE found nature easy to follow and difficult to drive. We usually wanted what she gave for our seeking, but we could seldom get exactly what we thought we wanted at the time. We wanted light. She gave us rectifiers." Thus Willis R. Whitney concludes an account of a research done under his direction in the effort to utilize the mercury arc as a source of light. New facts learned about the mercury arc during that study made its application in an altogether unexpected field, that of an alternating current rectifier, much more useful than as a luminous source.

This experience of Doctor Whitney's is a common one in research, as well as in other forms of searching. Saul the son of Kish, set out to seek his father's asses, and found a kingdom instead. Columbus sailed westward in search of the Indies, and found a New World. It was in search of a new material for pyrometer tubes that the rustless iron now used so much in architecture was discovered in the Krupp laboratories. The fermentation industry, founded to make acetone for powder manufacture, soon saw the day when the by-product of their process, butanol, became the most desired one because of its usefulness in lacquers. It is upon develop-

ments made for, and first used in, a transcontinental telephone line that radio broadcasting and the radio telephone, as well as the whole of the talking-movie art, rest. The telephone itself is a by-product of the effort to make a harmonic telegraph. Sir Humphry Davy said, "I thank God that I was not made a dexterous manipulator, for the most important of my discoveries have been suggested to me by my failures."

So it is that the successful searcher is usually the one who follows up the clues that nature gives, even though they do not go exactly in the direction desired. They may lead to something that is even better than the object sought for. But here a certain amount of judgment has to be used sometimes, so far as the industrial research worker is concerned, to keep from being led off along lines too remote from the field of activity of his employers to justify following them. Thus a man who is doing research for a dye manufacturer should not allow himself to go off on an investigation of some problem of the steam turbine, no matter how interesting or important it may appear to him to be. The free-lance or pure science investigator has no such limitation, however.

There is another form of by-product yielded by research that is perhaps still more important than the kind mentioned above, and maybe more important even than the direct products. Frank B. Jewett told about this by-product in an address that he made in 1931 to the members of the American Petroleum Insti-

tute. "I am, however, not at all sure," said Dr. Jewett, "that these direct results are the greatest benefits which have accrued to the industries from the introduction of industrial research over a long period of time. I rather surmise that if we had the means of measuring them, we would find that the indirect benefits were of even greater value. These indirect benefits have been largely the result of the gradual change in the point of view and methods of attack which have flowed out from the industrial research laboratory to all parts of the organization." There is no question that a major result of the application of the experimental method and the securing of definite facts to base decisions on has been to inject more straight thinking into the direction of industrial policies in general.

The exertion of this beneficial effect outside the research laboratory itself has been brought about in part by still another by-product of research. And that is the presence in other departments of an industry of men trained in the atmosphere of the research laboratory. "Where such transfers have been made," said Dr. Jewett in the address mentioned above, "they have almost invariably resulted in a rapid indoctrination in the departments of an appreciation of the power of the so-called scientific method as distinguished from the cut and try methods in the solution of new problems, or in the elimination of unexpected difficulties."

Any one can make the right decision if the facts are all before him. But the making of decisions in the dark

or on the basis of mere opinion, as is done when facts are lacking, necessarily gives results the correctness of which is too largely a matter of mere chance. So it is that any one who computes the value to his business of an organized research laboratory merely in terms of the dollars and cents return from the products that have been developed there reaches a figure that may be much too low; for the value of the by-products from research, although difficult to estimate in quantitative terms, is often a very substantial one.

CHAPTER XXXVI

“WHY DIDN’T I THINK OF THAT?”

THOMAS A. WATSON—the man to whom Alexander Graham Bell spoke that first telephone message, “Watson, come here, I want you”—told the following story about his old electrical mentor, Moses G. Farmer. After Farmer had read the first description of Bell’s telephone he could not sleep for a week, so upset was he at not having made that invention himself. “Watson,” said Farmer, “that thing has flaunted itself in my very face a dozen times within the last ten years, and every time I was too blind to see it.”

“Why didn’t *I* think of that?” is a question that many a man has asked himself after seeing some invention or discovery. Such things, once they have been actually realized, usually appear to be so absurdly simple that it is but natural for a fellow to wonder why he had never thought of it himself.

There is a statement of this common experience in one of the decisions of the U. S. Supreme Court. “Many things, and the patent law abounds in illustrations,” reads the decision, “seem obvious after they have been done, and, in the light of the accomplished result, it is so often a matter of wonder how they so long

cluded the search of the discoverer and set at defiance the speculations of inventive genius."

Of the various reasons why people ask the question embodied in the title, two or three may be mentioned. The first reason was suggested in a recent number of *Time*. There it was said that looking at an invention or a discovery after it has been made, when it can be seen in its entirety and with a clear view of its relationship to other things, is like criticizing a play or a literary effort after it has been produced, as distinguished from the actual ingenuity or imagination necessary to produce it. The English dramatist and critic, St. John Greer Ervine, said, "Anybody can take Shakespeare's plays to pieces, but only Shakespeare could put them together."

A second one of the reasons why people fail to think of new things is that even the most original of men has only a limited number of really new ideas. Mark Twain was a great humorist, but he sometimes repeated himself almost to the point of establishing a routine. His daughter, Clara Clemens, tells that when she and her sister Susy were small children, they used to sit on the stairs and listen to the broken bits of conversation coming up from the dining-room when dinner parties were given in the Clemens home. "We got into this habit," she writes, "because we used to hear so many peals of laughter in the distance that we would run to discover the cause of all the mirth. Almost always it turned out that Father was telling a funny story. Now, it hap-

pened a few times Father had told the same story on various occasions when guests were dining at the house and we had calculated that each time the meal was about half over. So we used to announce to each other, 'Father is telling the beggar story; they must have reached the meat course.' ”

A third one of the reasons why people fail to think of new things is concerned with the common lack of sufficient imagination to see the need for new things. "In thinking over past inventions," says S. M. Kintner of Westinghouse, "I cannot escape the feeling that has so frequently come to me, of how little we appreciated the need for many of them until after they were here, that is, the world to us appeared just as complete before as after these inventions were made."

Those just given are at least some of the reasons why it is that, as Paul de Kruif has suggested, men groped and fumbled for so many thousands of years without seeing things that lay right under their noses. And it is for exactly the same reasons that we are still groping to a very considerable degree.

CHAPTER XXXVII

PYTHAGOREANISM

IN the sixth century B.C. lived the Greek scientist Pythagoras. Pythagoras was an investigator who is credited with having made contributions to mathematics, to astronomy, and to physical science. Among other things, he is said to have paved the way for the Copernican theory by teaching that the planets revolved around a "central fire."

But what is said below is about another and less meritorious thing which Pythagoras did. He founded a secret society the members of which were pledged to reveal all their ideas and discoveries to him, and to him alone. That was his plan of doing a thing which, sad to say, some up-to-date explorers are guilty of, too—taking full credit for some things that he was not the sole originator of, if indeed he had anything at all to do with originating some of them.

The matter of giving credit where credit is due in the field of research is a difficult one, even when there is no modern Pythagoras involved, but when an honest effort is made to do so. This is especially true now that the era of the "cloistered inventor" is pretty largely over, and inventions or new developments are usually the product of a group of men rather than of one man

alone—if indeed there ever was a time when great inventions were made altogether single-handed. Under the conditions of organized research which exist to-day, it is next to impossible to tell just where all the ideas that are incorporated into an advance did originate. The final result is a composite product of the efforts of all those who worked on it, even though for sake of simplicity the patent application or the published paper covering it may bear but one or two names.

Dr. W. D. Coolidge, speaking of the development of ductile tungsten, a great advance which is commonly credited to him alone, said: "Whatever I may have done I did with the assistance of a staff of able workers, and...we all had the advantage of facilities for exhaustive research provided by a financially powerful and forward looking corporation." And Professor S. P. Thompson once said that "The seemingly useless or trivial observation made by one worker leads on to a useful observation by another: and so science advances, 'creeping on from point to point.'" There, in a nutshell, is the way new developments are made in a modern research laboratory. It is more like the taking of a sector by a detachment of men than the capture of it single-handed by a Sergeant York.

It has been said that even the greatest of inventors "stand almost without exception on lesser men's shoulders." The truth of the matter is that in these times no advance can be made by any one without standing on

other men's shoulders, some greater and some lesser. "All scientific work," said Professor Harold C. Urey, "depends on the careful work of our predecessors and co-workers and...our rapid advance in the sciences is due largely to the freedom with which we publish the results of our own work. A realization of these facts makes it difficult to take any really profound credit to ourselves for individual accomplishments."

Pasteur's experiments on dogs and other animals would not have been possible without the use of chloroform, the anesthetic effects of which he did not discover. Pasteur had a horror of useless suffering. Many men had a hand in the development of anesthesia, but it is said to have been five American experimenters, Long, Jackson, Wells, Morton, and Warren, who finally made it a practical realization. Neither could Pasteur have done his work without the microscope, nor without many other things developed by a host of other men. Banting could not have developed insulin, says J. B. S. Haldane, without the aid of a method of accurate blood sugar analysis. Analysis for blood sugar is a science that "has been brought to its present degree of efficiency by some sixty years of very persistent and rather dull work in hundreds of different laboratories." The contributions of James Watt to the steam engine were made possible by the prior experiments of others and by the great improvements in iron making that marked the final quarter of the eighteenth century. It is said that the spinning machinery of 1857 was "a

compound of about eight hundred inventions." Charles F. Kettering, who put electric lights on the automobile, has said that without the tungsten filament lamp he could not possibly have done it. Marconi in developing the wireless telegraph stood on the shoulders of many men, three of whom were Lord Kelvin, Clerk Maxwell, and Hertz. Radio broadcasting had to wait on the vacuum tube, and television on both the vacuum tube and the neon lamp. Even Newton's great hypothesis was based not, as is commonly supposed, upon his own observation of the falling of an apple from a tree, but upon the prior studies of another investigator, Huygens.

So it is that discoveries and inventions have a way of coming along when the time for them is ripe, and not often before, which is perhaps why it is that even the most fundamental of discoveries are sometimes made in more than one place at once. Thus the principles of electromagnetic induction were discovered simultaneously by Faraday in England and by Joseph Henry in the United States. Mendeléeff and Lothar Meyer made similar classifications of the chemical elements at about the same time. From 1842 to 1847 the generalization of the conservation of energy was arrived at independently by J. R. Mayer, by K. F. Mohr, and by H. von Helmholtz, in Germany, and by L. A. Golding in Denmark. Hence it is difficult to find out in some cases who the originator of the essential feature of a new thing really is. And this is one of the reasons why

it happens that for some inventions different countries give the credit to different men.

Nevertheless, men engaged in research and in science generally are jealous in the matter of allocation of credit. The principle of "finders keepers" has always been thought to apply to discovery, whether in geography or in science. In science, a discovery or a new idea is a child of the mind, and the father of a worthy child always wants to be known as such. He does not want some other man to filch the credit from him. The personal gratification and pride in having made a contribution to science or industry and the recognition of it by others, particularly by fellow researchers, constitute a definite part of the reason why many good men follow research, instead of some other endeavor which in a financial way might reward their efforts much better. "Rather would I explain the cause of a single fact than become king of the Persians," said Democritus.

This pride that each research worker has in the result he achieves or helps to achieve is one of the reasons why it is desirable to publish the findings of research, whenever that is possible, and to allow each result to be published in the name of the man or the men who contributed it. Whenever the head of a department, or any man who happens to be in a position which enables him to organize the effort of other investigators, "pythagorizes" the results and publishes them or patents them in his own name alone, he is fostering

dissatisfaction, and even resentment, among the men associated with him. At the same time he is reducing their loyalty and morale, and consequently lowering their efficiency. "Nothing is more disastrous for a laboratory," said C. E. K. Mees, "than a director who claims credit which rightfully belongs to his men."

The contributions that any one man or group of men can make are small enough at the best. If any of those few contributions of an investigator are taken from him, then he is poor indeed, and he is likely to feel his poverty enough to become seriously discouraged. So it is that a Pythagoras may well be a very bad man for an ambitious and enterprising research worker to be associated with.

CHAPTER XXXVIII

REMUNERATION

HERBERT HOOVER in his Edison Day address in 1929 said: "Our scientists and inventors are among our most priceless national possessions. There is no sum that the world could not afford to pay these men who have that originality of mind, that devotion and industry to carry scientific thought forward in steps and strides until it spreads to the comfort of every home; not by all the profits of all the banks in the world can we measure the contribution which these men make to our progress."

In the light of Mr. Hoover's words, what should the remuneration of a research worker be? On the answer to this question there are two schools of opinion. The one was expressed by Pasteur to Napoleon III when he said that a man of science would consider that he lowered himself if he tried to make his discoveries and their practical applications a source of personal profit. "I could never work for money," Pasteur declared to Lady Priestley also, "but I would always work for science."

The other opinion was expressed by Charles F. Kettering in an address before members of the American Chemical Society in 1927. "I was taught," said Mr. Ket-

tering, "that a scientist is a man who works at his subject for the sake of the subject alone, and that a man who works on a scientific project with the idea of selling it has no right to be associated with science. I have since learned that a bank account in the black is the popular applause of a scientific accomplishment."

On first thought these two opinions appear to be directly in conflict. But perhaps they are not as conflicting as it appears. When Humphry Davy was urged to take out a patent on his safety lamp, this was his response: "I never thought of such a thing; my sole object was to serve the cause of humanity; and if I have succeeded, I am amply rewarded in the gratifying reflection of having done so. I have enough for all my views and purposes; more wealth could not increase either my fame or my happiness. It might undoubtedly enable me to put four horses to my carriage; but what would it avail me to have it said that Sir Humphry drives his carriage and four?"

Perhaps one of the reasons why the first opinion—that which says that a man of science should not work for money—is held by some research workers is embodied in the above quotation from Sir Humphry. Reference is to that part of what Sir Humphry said which suggests that he did not need to do research for money, or that he was not dependent upon any return from his researches either for his living or for the ability to continue his investigations. If that is not the reason why such an opinion *is* held, it is at any rate a

reason why it is *possible* for it to be held; for workers in science must live, of course.

Up until the time when some research began to be done for its own sake alone, or by men who had no other employment or no other means of support, it was done to a large extent by men such as Charles Darwin, who was an English gentleman of wealth and leisure, or by such men as Humphry Davy or Pasteur whose support was provided by some educational institution. It is not to be wondered at that those men, wrapped up in endeavors which held their intense interest, should not care to take the time and trouble to give the products of their researches to the world in a usable form, as that is sometimes a much bigger job than the research itself. The statement of Louis Agassiz, "I have no time to make money," is a perfectly natural one to come from such a man, since, because his support was provided by Harvard University, he did not need to direct his researches with a view to making them pay.

In his address as outgoing president of the American Physical Society, delivered in Boston at the end of December, 1933, Paul D. Foote related how among present-day physicists the academician too often looks down upon his "tainted" brother in industry, while he prides himself upon his "pure" research. But, said Dr. Foote, any experimenter who thinks that he is searching after truth for truth's sake alone is laboring under a delusion. "Actually the physicist is in the research busi-

ness because it is a pleasant means for making a living; because he enjoys his work and the prestige accorded him by his superior officers or by his society when he presents a creditable paper."

As a challenge to those academic physicists who believe the "fiction" that they are seeking truth merely for truth's sake, Dr. Foote proposed the following experiment. Let the papers submitted to the *Physical Review* be passed upon by an editorial board, and then let all those which that board approves be published under the sponsorship of the American Physical Society, but entirely anonymously so far as the author and the institution with which he is connected are concerned. Truth would be served none the less by that means. But, on the basis of his own long experience as an editor, Dr. Foote thought that the effect of such a rule would probably solve the then current financial problem of the *Physical Review*, which arose from having too many papers to print.

Why then should any man whose research does have to support both itself and him by its results, or by the return from the service it renders to society, consider that he is thereby reduced to a lower level than an investigator who is not in that position? Is not the question of real importance here whether the main objective of the workers is to serve society through research or to make society pay him a tribute? Certainly, in research as elsewhere, the laborer is worthy of his hire. The truth is that in most cases a laborer has

to have his hire—either from a direct or from an indirect source—in order to live and to continue to work.

But the mere fact that a research worker has to be remunerated in some way for his work, does not make it necessary at all that he become a grasping pirate such as some geographic explorers were—Sir Francis Drake, for instance. That is neither justified nor necessary. But surely it is right that there be a system of remuneration better than that which caused the great Isaac Newton at one time in his life to be so poverty-stricken that he had to ask to be relieved from paying his weekly dues of one shilling to the Royal Society; or than that which allowed Jean Henri Fabre to be paid only about three hundred dollars a year, “less,” as Fabre himself said, “than a groom in a well-to-do household.” It used to be so, and maybe is so still to some degree, as Arthur D. Little wrote in “The Fifth Estate,” that “Carrying a lantern is often less remunerative than carrying a hod.”

Adequate remuneration for a successful research does not constitute a burden to society at all. A new development of importance performs a service for many, many people, and perhaps for every one. Such a simple thing as the microphone used in radio broadcasting and in the making of sound pictures, for instance, touches the lives of every single person in the country. So it is perhaps not too much to assume that it has already contributed a cent's worth to the enjoyment and to the service of every one in the country. And, if each one

were to have contributed in return one cent to the men or to the organization which made the development, the total would have been more than one million dollars. Even one-tenth of a cent per person would have amounted to a hundred thousand dollars. Surely on such a basis society can well afford to give a reasonable remuneration to those who, as Herbert Hoover said, make its forward steps and strides for it.

The problem of adequately remunerating those who are doing research with the direct aim of making advances which will be of value to society, and the other problem of making the results available to people in usable form, are both being solved at once to a very considerable degree by a twentieth century institution—the organized industrial research laboratory. Such an institution can pay a suitable wage to the workers within it, even though not every one of them should happen to be successful in making a valuable advance of some kind. Furthermore, an organized research laboratory, aided by the industry by which it is supported, can and does perform that other very valuable service of putting the results of research into a practical form which can be useful to people. One consequence of such organization, as expressed by E. E. Free, is “that scientific men who commit the once heinous sin of making money by applying their science to practical things now sit in seats of respect and authority exclusively reserved three decades ago for those whose science was ‘pure.’”

The problem of what is a suitable wage for a research worker thus becomes the same as for any other worker of similar rank. "On the whole, the best plan, probably," says C. E. K. Mees of the Eastman Kodak Company, "is to pay men by fixed salary, advancing their salaries in proportion to the quality of their work." Dr. Mees suggests further that such advancement ought to be made "whether that work results in direct financial gain or not." This latter part of Dr. Mees's suggestion is made on account of the considerable element of luck in research, which may cause the efforts of some of the best workers to be directly productive of very little in the way of useful results; although, truth to tell, it is impossible to know right at the time what results will ultimately find the most useful application. As J. B. S. Haldane has pointed out, the importance of a discovery is sometimes not realized until after the discoverer is dead. "When Richardson discovered the laws governing electron emission from hot metals, he did not, presumably, suspect that he had made wireless telephony practicable." Consequently it is not possible to remunerate each research worker in direct proportion to the value of his contributions. It was partly on this account that L. A. Hawkins of the General Electric Research Laboratory warned, "Beware the Bonus. Large bonuses for inventions are fatal errors, conducive to jealousy and immediately destructive to teamwork. Practically, in a laboratory enjoying true coöperation in its staff, nearly every important de-

velopment represents the effective contributions of many workers.... Meritorious work should be recognized in salary."

What the amount of that salary should be is determined by the same considerations as those which apply to other workers. What it is now on the average—or was in 1931—is suggested by Professor Alfred H. White of the University of Michigan in the February, 1932, number of *Industrial and Engineering Chemistry*. There he gives the results of a survey of the earnings of chemical engineering graduates conducted by the Committee on Chemical Engineering Education of the American Institute of Chemical Engineers. The results showed that in general the salaries of those chemical engineering graduates who are engaged in research are comparable with those who are doing other work for which men of chemical engineering training are qualified. In fact, the salaries of those engaged in research are a little better on the whole than the average of all those reporting. Representative figures from the survey are given in the table opposite.

These figures show that, of those research workers whose preparation has included two years of graduate work, one-half make salaries of \$5,400 or more. In evaluating these figures remember that they refer to young men who are only eight years out of school. Compare with the figures given also the fact that, according to the results of a survey published in *Technology Review* in 1931, the average annual salary of

SALARIES OF GRADUATES IN CHEMICAL ENGINEERING

	<i>No Graduate Work</i>		<i>Two Years' Graduate Work</i>	
	<i>Research Group</i>	<i>All Groups</i>	<i>Research Group</i>	<i>All Groups</i>
A. Median of Annual Salaries:				
5 years after first degree	\$2,700	\$2,700	\$2,700	\$2,700
10 years after first degree	3,800	4,100	5,400	3,800
B. Lower Limit of Upper Salary Quartile:				
5 years after first degree	3,200	3,300	3,000	3,300
10 years after first degree	5,400	5,000	6,200	5,300

graduates of Massachusetts Institute of Technology under thirty-five years of age is \$4,000. Compare with it further the fact that less than one American wage-earner in twenty-five makes as much as \$5,000 a year.

Thus it appears that industrial research workers are being paid salaries comparable with those received by men of similar capabilities in other fields of endeavor, which is as it should be. But the real reward of the up-to-date explorer is the same as that of other creative workers. And that is the satisfaction which comes to him from the successful doing of creative work.

CHAPTER XXXIX

PENALTIES OF PIONEERING

THE great naturalist, Louis Agassiz, said once that every new scientific truth passes through three stages. First, men say it is not true; second, they say it is hostile to religion; and, third, they say that every one has always known it.

Perhaps these reactions are not quite so pronounced or well-defined now as they used to be, but it is still true that the man behind any new development must not only make the development itself, with all the difficulties involved in doing that, but he must also batter down a barrier of conservatism before his idea reaches acceptance. It is still true, as Maurice Holland has said, that "the history of invention is filled with heartbreaks."

One of the chief weapons of conservatism, said Edwin E. Slosson, is ridicule. "If you want to know what line human progress will take in the future," wrote Dr. Slosson in his *Creative Chemistry*, "read the funny papers of to-day and see what they are fighting." People laughed at Fulton's steamboat, at Stephenson's locomotive, and at Franklin's experiments with electricity. They laughed at Napoleon for encouraging experiments on getting sugar out of beets, and at the

Wright Brothers for trying to fly. In the same way, also, people scoffed at Thomas Jefferson for daring to think that the land west of the Mississippi was worth anything, and at William H. Seward for wanting to make Alaska a part of the United States. "Seward's Ice Box" they called it.

People certainly laughed a-plenty at the early automobile too, and few there were who had any idea that the automobile would ever amount to anything. Adjutant General Corbin, writing in the *Brooklyn Eagle* about 1900, expressed some opinions of that time:

I do not think the automobile is going to be popular for any length of time with our fashionable people. During my recent stay in Newport I met a number of wealthy people who had purchased automobiles early in the season, and I asked them how they liked these novel vehicles. Almost without exception they told me that they did not think the fad for them would last long. They said the automobile would never take the place of the horse with the fashionable set. The fact of the matter is, the swell woman does not appear to as great advantage in an automobile as she does behind a pair of fine horses.

It must be admitted that new things are sometimes so imperfect that pessimistic opinions about their future appear to be well justified at the time on the part of those who can not see beyond the thing as it then is. And the motor car was no exception to this. One thing that has had a large influence in making the opinions about the automobile quoted above turn out

to be wrong is that, thanks to a great deal of experimentation since the time of those remarks, the motor car has changed very much for the better; but the horse is just the same as he was in 1900.

The opposition to the new has often gone much further than ridicule or mere passive non-acceptance, of course. So great used to be the hostility to any change from accepted views or beliefs that Leonardo da Vinci did not even dare to write down his thoughts or his discoveries in conventional form, but instead he used reversed or mirror writing for his records. Galileo Galilei, who dared to prove that some of the ideas of his day were wrong, was forced to "abjure, curse, and detest" all such "errors and heresies" under threat of torture. Jenner's ideas about vaccination against smallpox were strenuously opposed and grossly misrepresented. Cartoons were published showing people with cow's heads and horns to suggest what might be expected to result from vaccination, and in churches vaccination was preached against as a wicked thing. The great preacher, Cotton Mather, who voiced his approval of inoculation against smallpox, was attacked by having a hand grenade thrown through his window. So apparently the obstructionist methods of modern racketeers did not originate with them.

The first Hargreaves spinning apparatus was twice destroyed by mobs, and factories which used Arkwright's spinning frames were frequent subjects of attack. The early grain reapers and binders used often

to be found burnt up in the fields. When bathtubs first began to be used in the United States in the 1840's they were attacked in the papers as extravagant and undemocratic, and doctors even denounced them as dangerous to health. The reward that Thomas A. Edison got for his first invention—a telegraph repeater—was to be fired from his job in a Memphis telegraph office.

But nowadays the resistance to the new, so far as people in general are concerned at any rate, is not so great as it used to be. "No longer," says Arthur Pound, "is the innovator considered a heretic or a crank." One of the reasons why there has been a change for the better in this regard is that in recent years new things of practical value to people have been coming along so rapidly that they have gradually learned to accept them, sometimes without question, and even to look for others. Another one of the reasons is that more publicity has been given to the research from which new developments come.

But, unfortunately, not all this publicity has been of a commendable character. Not appreciating the difficulties of research and the long time required to make satisfactory progress toward an objective, people have sometimes been led by stories of the far-from-completed work of some investigator to believe that its results were ready for use. "We have an immense amount of popularization of the results of science," said Justus von Liebig, "but it is to be feared that much of this is too

easy, shallow and misleading." Too often stories of research give the impression that great things have been done by some mysterious or magical means, rather than by mere patient, prosaic plugging, coupled as well with some groping and stumbling. The fostering of such ideas can easily do harm both to those who support research and to those who are users of the results of research. There is also no question that the advertising value of research has been seized upon and commercialized in unwarranted ways, such as for suggesting that some minor and unimportant change in a product, or some mere "gadget," is an outstanding improvement which is the result of a great deal of costly research.

The penalties of pioneering, although not as great to-day as they used to be, are still very real. They arise from the very nature of pioneering, which of necessity demands expeditions into regions beyond the present outposts of civilization or knowledge. As Fabre said, "The man who thinks of the future is always out of style to-day." The pioneer must expect to encounter rough, difficult going; to meet with obstacles that appear to be unsurmountable and impassable; to have difficulty selling the idea that his expedition is worth the financial expenditure required to support it; to find it hard sometimes to keep up the spirits of those toiling forward with him; and, finally, to get people to accept the results of his discoveries, once they have been made. Any one who is not prepared to meet all these

obstacles and to pay the penalties involved in overcoming them had better not try to do research; for it is still true, as Charles F. Kettering has said, that the price of progress is trouble.

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A SELECTED LIST OF PUBLICATIONS ABOUT RESEARCH

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